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AMERICAN ACHIEVEMENTS AND AMERICAN FAILURES IN PUBLIC HEALTH WORK[1]

PUBLIC health work in America began early in the eighteenth century with the introduction into New England of the Oriental inoculation for small-pox by Boylston, and has achieved world-wide renown early in the twentieth century with the scientific sanitation of the tropical Isthmus of Panama by Gorgas. The educating, organizing and equipping with sanitary machinery of a swiftly growing population, at first sparse but later sometimes intensely congested, and always fluid and unstable under the pressure of migrations and immigrations such as the world has never seen, is in itself a great achievement. And when that population is, like ours, compounded of all the races of mankind, lodged in a new environment and subjected to an unfamiliar and quickly changeable climate, public health work becomes exceptionally difficult. Nevertheless, under leaders like Boylston and Waterhouse, Shattuck, Walcott and Billings, and Reed, Lazear and Gorgas—to whom we may now proudly add the name of Strong—sanitary information has been gathered and spread abroad and applied; vital statistics have been collected and studied; sanitary libraries have been formed; boards of health have been organized and directed; public health laboratories have been established; and epidemiology and other branches of sanitary science enriched and extended.

[1] Address of the President, American Public Health Association, Rochester, N. Y., September 7, 1915.

To these achievements various arms of the federal government have materially contributed, *e. g.*, the U. S. Public Health Service, the Department of Agriculture, the medical corps of the navy, and, especially, the army medical corps which through the splendid and eminently scientific labors of Reed, Lazear and Carroll and Carter and Gorgas have solved the deepest mysteries, and overcome the worst difficulties, of tropical sanitation. Drs. Blue and White and their associates of the U. S. Public Health Service have likewise earned the honor and gratitude of mankind by their proof that under proper administrative measures the bubonic plague of the Orient and the yellow fever of the Occident may alike be held in check in our own country, while, as we are meeting to-day, Strong, formerly a government official, but now in Serbia and Montenegro, representing various more private foundations, is winning fresh laurels for American public health measures. Thus the bold and brilliant epidemiological work of Boylston and Waterhouse which marked the beginning of experimental preventive medicine in the early eighteenth and early nineteenth centuries, has been continued and extended in the late nineteenth and early twentieth by their still more scientific successors. Some of our state and city boards of health and some of our private institutions and laboratories have likewise lent invaluable assistance, as for example the Massachusetts State Board of Health, the New York City Board, the Rockefeller Institute, and many others. Meantime, less original but not less enthusiastic and faithful work has been done by various associations for the promotion of the public health, such as the American Public Health Association, and the now happily numerous anti-tuberculosis societies, the organizations for the promotion of school hygiene, for the prevention of infant mortality, and the like. We have also, at last, in addition to a small but increasing number of creditable state, town and city boards of health, what is virtually a national board of health, namely, the U. S. Public Health Service—a highly scientific and efficient arm of the federal government, well organized, well equipped, and provided with a staff of able and devoted investigators.

Up to 1886, *preventive medicine* chiefly in the form of vaccination was the principal weapon for the promotion of the public health, and a long step forward was taken when in that year with a reorganization of the State Board of Health of Massachusetts, sanitary engineering became a recognized and indispensable branch of public health science and public health service. Since that time *preventive sanitation,* and particularly the sanitation of towns and cities in respect to water supplies, milk supplies, ice supplies, sewerage, garbage disposal, street cleaning, the heating and ventilating of public buildings, the smoke nuisance, and other environmental factors of public health or ill-health, has come to receive close attention and treatment. In the further development of preventive medicine and preventive sanitation, public health laboratories for the quick detection of dangerous infections have rapidly been installed almost everywhere in the more progressive American cities and towns. We have begun the medical and sanitary supervision of schools and school buildings. We have invented and put within the reach of all but the very poor, the most complete, convenient and salubrious heating and ventilating appliances in the world, for houses, theaters, halls, hotels and workshops. We have made ice, once a luxury for kings and emperors, a universal solace for all classes in hot weather. We have perfected and extended enormously the

preservation of foods by cold and by canning, so that seasonal food scarcity is almost unknown. We have invented and cheapened rubber clothing, and especially rubber overshoes, as a protection against our almost tropical rains. We have applied machinery to the manufacture of abundant and better and cheaper shoes and clothing. We have proved by experiment with a national spitting nuisance the possibility of sometimes controlling unsanitary habits by education and reasonable sanitary ordinances.

And yet—on the other hand—we have thus far failed to achieve many much needed sanitary improvements. Our water supplies are to a large extent either in good condition or on the way to improvement, but our sewage disposal systems are still in many cases far from satisfactory. In this respect the parallel between the individual and the community is close, for while many intelligent persons attend carefully to the water they drink, most are comparatively careless about their excretions, regarding as negligible that frequent and regular and complete output of the body wastes which is no less necessary for the conservation of health than is the intake of wholesome food and drink. The most flagrant failure in American sanitation to-day is the almost universal lack of public convenience or comfort stations in American cities and towns. The stranger within the gates of most American communities seeks in vain for any public sanitary conveniences. If he is well-dressed, he must be referred to hotels or other semi-public buildings or, if poorly dressed, to saloons or railway stations or other semi-private or public-service places. Some three months ago the leading newspaper of one of the proudest and most progressive cities of New England, which has since rejoiced to find itself "in the hundred thousand class," announced that its

> first public sanitary . . . was opened Saturday morning, and will be open daily hereafter from 6 A.M. until midnight. The opening . . . marked the end of ten years of effort to get such a comfort station built.

Failure like this to provide proper public toilet facilities for our towns and cities is to fail in one of the very elements of public health.

We have also failed to reduce typhoid fever as far as we should have done in America. Of late much progress has been made in the right direction but we need to remember that it is the last step that arrives, and we have always failed to attend closely enough to the single, as well as to the seemingly final, case. Like nature we are often "so careless of the single life, so careful of the race." We have failed likewise to reduce as far as we should have done American infant mortality. Here undoubtedly our hot weather works against us, but so also do our milk supply, which can be and ought to be rendered safe by pasteurizing, and our parental ignorance and incompetence, which can and should be lessened by education and the aid of public health nurses. We have as yet, and in spite of ample knowledge, failed to make our American milk supplies what they should be. This is partly because we have been too timid to insist that good milk not only costs more to make but is worth more for food, and must therefore be paid for, and partly because we have not yet taught the public as we should that the only safe milk is cooked milk, and for infants, milk that is pasteurized—preferably in the final container. I have myself lived through the last years of the period—now happily remote—when no milk was pasteurized by anybody; through the next in which only pioneers like Nathan Straus preached or practised pasteurization, while many if

not most, physicians, deprecated the practise; through the one following, in which the scales began to turn in favor of pasteurization; and into the present when almost no one fully informed on the subject actively opposes pasteurization. And yet, even to-day, some physicians are short-sighted enough to tolerate if not to recommend the general use of raw milk, which still constitutes the great bulk of the milk used by infants and adults all over the land. Such use of raw milk we must count as long as it lasts one of our worst public health failures.

We have also failed thus far to pay proper attention to our American climate, which has been well described by a popular writer as "polar-tropic." Placed as we of the United States are on the latitude of southern Europe, Persia, and the northern parts of Africa and Arabia, we are exposed alike to the stimulating and dangerous sunburn of the south and the cold dry winds driving down upon us from the continental area of Canada and the high north. In this fact perhaps we shall some day find one of the principal causes of that eager, strenuous, often nervous and sometimes excited, condition which we may call "Americanitis."

Another conspicuous failure is our rural and industrial hygiene and sanitation. With vast regions given over to rural life, and with other regions called industrial, small in area but teeming with life and noise, we have as yet only touched the surface of the public health problems involved. The same is even more true of the problems of alcoholism and venereal disease. Here we shall probably learn by comparison with the spitting evil that it is easier to overcome habit than to conquer appetite, and we must be prepared for delays and disappointments yet without giving up hope.

Some streets of most American cities are often disgracefully dirty and untidy. Horse-dung and other dirt, dust, papers, fruit skins, old hats, abandoned umbrellas, discarded shoes and the like are too often seen lying about our streets. Yet these same streets are the principal playgrounds of the poor, and ought for every reason to be kept scrupulously clean. We have devised excellent apparatus for heating and ventilating halls and houses in our polar winters but have neglected the almost equally important problem of cooling habitations and public buildings in our tropical summers. We have not done all we might do for the prevention of blindness, of tuberculosis or of cancer. Our vital statistics are not yet either complete or trustworthy; our health boards are too often loaded up with political refugees, political doctors, and ignorant or incompetent laymen. Our health officers are frequently untrained, ill-paid, or only part-time employees of a no-time board.

But above all I must put our almost complete neglect of *preventive personal hygiene*. From 1720 to 1886 we had little to show in public health work beyond vaccination for small-pox—the fundamental procedure of *preventive medicine*. To this, which has since expanded immensely in various directions, we have added *preventive sanitation*, by which I mean the purification of water and sewage and milk, the control of mosquitoes to guard against malaria and yellow fever, improved housing, and many other fundamentals of a sanitary environment. But we have not yet even begun to demand that study and care of the *individual* which is the most fundamental of all public health problems. We have paid little or no attention to the prevention of overeating, overworking, overdrinking, deficient exercise and deficient sleeping, to family hygiene, and the hygiene of

special organs such as eyes, ears, bowels, teeth, nose and feet—all of which I propose to group together under the term *preventive hygiene.* We have achieved much in preventive medicine and preventive sanitation, but we have as yet failed for the most part in preventive hygiene, which is very likely the most important of all. Here, therefore, we may reasonably expect the greatest progress in the nearest future. Rightly studied, preventive hygiene will include personal, domestic, family and social hygiene. It will deal with celibacy and marriage, with sanitary house-keeping, with the high cost of living, with food economy, with domestic service, with child hygiene, and with the proper conduct of mature and elderly life, as well as with the manifold aspects of strictly personal hygiene. It will in the future play perhaps the principal part in solving many of the problems of American life, health, prosperity and happiness.

Our whole teaching of hygiene and sanitation has been grossly neglected, and our teaching of physiology on both the higher levels and the lower has never emphasized as much as it should have done its practical hygienic applications to the conduct of life. Even our best medical schools have paid but scant attention to these subjects, while the instruction given in the public schools has hitherto suffered from uninformed school committees and half-informed teachers. The best teaching of today is to be found, not in the text-books or the schools, but in the leaflets issued and distributed by certain leading boards of health and life insurance companies. Surely this is a scholastic reproach which should not be allowed to stand.

In conclusion I desire to express my appreciation of the honor conferred upon me by the gift of the office which I hold. The American Public Health Association is today a splendid force in the land. Its *Journal,* under the able editorship of our devoted and faithful secretary, Professor Gunn, is worthy of the great and truly international body which it represents. It is your duty and mine to strengthen Professor Gunn's hands, to increase our membership and help on the good work which is being done. If the Association continues to grow in numbers and in influence along the broad paths already marked out, remaining always democratic rather than bureaucratic, it will be worthy of the great name—"American"—which it bears. Two of our component members, the Dominion of Canada and the Republic of Mexico, are bearing the heavy burdens of war—the one foreign, the other civil. Both have the liveliest sympathy of their confreres in this association. It may seem to some as if, under the shadow of a war characterized as never before by the destruction of life, efforts for its conservation through hygiene and sanitation must be of little moment. But it is not so. "After the clouds the sun": and we believe that after the present bloody conflicts are ended—and may that time quickly come—the races of mankind will turn, as never before, and with new longing, to the nobler pursuits of life, liberty, health and happiness. When that better day dawns the eternal principles underlying the conservation and promotion of normal life and health will once more move and quicken the nations as sunshine warms and quickens the earth after storm. Meantime, we must make ready for active and intelligent dealing with the thousand new and pressing problems which the present conflicts are certain to bring before us. And to this task we turn with cheerful courage.

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A SIMPLE METHOD OF INDICATING GEOGRAPHICAL DISTRIBUTION

THERE are two ways in which the distribution of plants and animals on the surface of the earth may be expressed. The first is in terms of countries or provinces or their subdivisions. To this there are several objections: (*a*) The boundaries of a country or province or county are frequently very irregular and do not follow any definite direction. These areas are, moreover, as a rule, very unequal in size. (*b*) The boundaries of countries which have a different form of government are unstable and are liable to change. This has frequently happened already within the memory of men still living and is likely to happen again in the near future. (*c*) The method is only applicable to land areas. It can not be applied to the fauna and flora of the oceans.

The second method is one which has not been much used so far but is much more scientific and exact. It is that of indicating the range of a plant or animal in terms of latitude or longitude. However important international boundaries may be from the human standpoint they have no meaning to plants and animals—unless indeed they should happen to coincide with some natural boundary, such as a mountain range or an expanse of water. But the parallels of latitude and the meridians of longitude are so numerous that it is difficult to remember the particular countries traversed by any one of these in its course. While most people know that the forty-ninth parallel forms the northern boundary of the western half of the United States, very few indeed could name the states traversed by the fortieth parallel.

The method suggested here is a modification of the second of the above. It is proposed to divide the whole earth's surface into a series of areas bounded by the parallels and meridians. Each of these areas will be more or less rectangular, but only two of the four sides will be actually parallel. The size of the areas will gradually diminish towards the poles, but those within the same latitude will be of equal area. Each "merosphere" will have a definite number attached to it and will be capable of division into smaller areas, each of these ultimate units measuring one degree of latitude by one degree of longitude. The size of these proposed primary areas or "merospheres," as I have called them, will be a matter for legitimate discussion. If too small they will be so numerous as to have no advantage over the method of expressing distribution in terms of latitude and longitude. If, on the other hand, they are too large they will be useless for indicating distribution. The actual size proposed here measures six degrees of latitude by nine degrees of longitude. The reason for the adoption of these figures is twofold. It is true in a general sense that isotherms or lines of equal mean temperature run for the most part east and west unless where deflected by mountain chains or sheets of water when they may run in any direction, even north and south. It is also true, in a general sense, that the temperature falls steadily from the limits of the tropical region towards the limits of the polar regions. While there are wide variations in different continents, I have taken the rate of fall as being on the average about $1\frac{1}{2}°$ F. for every parallel of latitude. Six degrees of latitude will therefore correspond to about 9° F. or 5° C.

If the width of the proposed areas were 6° of longitude instead of 9° these areas in the neighborhood of the equator would be approximately squares, but would be narrow towards the poles. By making the width of the proposed areas 9° instead of 6° squares will occur about midway between the equator and poles and the number of areas or "merospheres" will be proportionately reduced.

The details of the proposed scheme are as follows: Beginning at the equator, the northern half of the earth is divided into fifteen parallel belts, each comprising 6° of latitude. These are numbered consecutively from N 1 to N 15. The southern hemisphere is divided similarly into belts S 1, S 2, etc. Each belt is divided into 40 divisions beginning at the meridian of Greenwich, the numbers running consecutively westwards until the meridian of Greenwich is again reached. As mentioned above, each division comprises 9° of longitude.

The belt number can conveniently be distinguished from its division numbers by a dot placed between them. Thus the state of Georgia would be included for the most part in N 6.10 with the extreme eastern area in N 6.9. The island of Tasmania would be comprised in S 7.17 and S 8.17.

Subdivided in the above manner, the United States would be comprised in belts N 5 to N 9 and would be contained in 29 divisions.

The above-defined areas, though large, are sufficient to indicate in a general way the distribution of plants and animals. But where greater exactitude is required, as, for example, where it is desired to indicate the most southerly point reached by a typical northern species or *vice versa* they are rather vague. Accordingly each "merosphere" may be again divided into smaller areas, each consisting of one degree of latitude and one degree of longitude. As will be seen from the annexed figure the east and west sub-belts are numbered from 1 to 6 and the nine strips running north and south are marked from A to I. By the com-

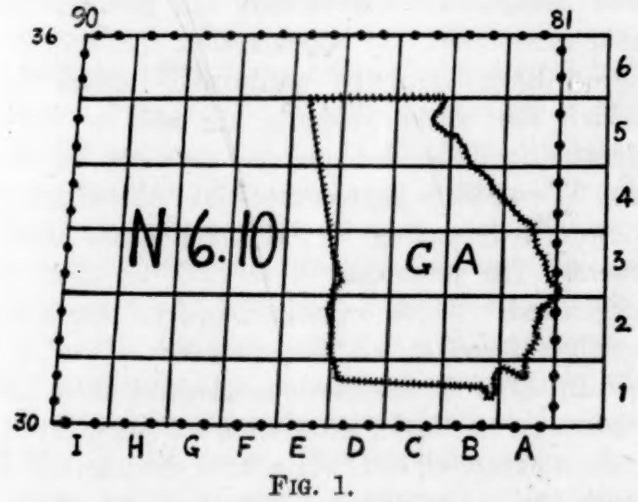

FIG. 1.

bination of a number and letter the position of each of the 54 subdivisions can be indicated, thus N6.10.5E will include the northwest corner of Georgia. One advantage possessed by the system of numbered areas herein described is that it can be used alone or in conjunction with the name of the state or province where fuller information is desired. Thus Georgia (N6.9.3I) will indicate the area around Savannah. Altogether this state contains twenty-four of these smaller subdivisions.

If some such scheme of uniform subdivisions were adopted in the case of a "flora" which deals with a single state or country the separate units of adjacent states or countries could readily be grouped into the larger areas mentioned.

It must, of course, be borne in mind that some such scheme as the above is intended to indicate only the *geographical* range of species. The division of the earth's surface into *biological* provinces, each with its characteristic assemblage of plants and animals, is a different matter. Before we can divide the earth satisfactorily into biological areas we must know first the actual limits of distribution of each species as well as something of the climatology in the widest sense of the proposed areas. At present we have not amassed sufficient data to enable us to make these wide generalizations, though several persons have made the attempt with varying success.

The need for some such division of the earth's surface, as I have attempted to outline above, is not so evident in the United States as it is further north. Such terms as Labrador, Alaska and Ontario, referring as they do to very large areas, are too indefinite to indicate with exactness the distribution of the fauna and flora. Labrador formerly had a different meaning from what it has at present, the greater part of the territory formerly called by that name being now part of the province of Quebec, and even yet the boundary line is not properly delimited.

The distribution of a certain species is given in the "North American Flora" as "Nova Scotia to Georgia, Tennessee and Michigan." Presumably it occurs in Ontario, as a line drawn from Nova Scotia to Michigan will pass through the province of Ontario. But it is not by any means certain whether the species occurs in the southern parts of New Brunswick or Quebec. According to the method outlined above the distribution would be N6.9–10, N7.8–10, N8.8–10.

Another species is mentioned as extending "from Newfoundland to Florida, Alabama and the Mackenzie." As this river has a course of over 1,000 miles in length it will be

evident that it is a somewhat indefinite boundary line.

To express the distribution of any species it ought to be sufficient to enumerate the divisional numbers of the areas in which it occurs and its ultimate limits to the north and south and in an easterly and westerly direction. Range of altitude in each division, or at any rate in each belt, is of as much importance as range in latitude.

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THE COMMITTEE OF ONE HUNDRED OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

REPORT OF THE SUBCOMMITTEE ON THE SELECTION AND TRAINING OF STUDENTS FOR RESEARCH

At the meeting of the Committee of One Hundred of the American Association of Scientific Research in April, 1914, the chairman was authorized to appoint a subcommittee on the Training and Selection of Students for Research. He subsequently appointed the following members: Professor R. A. Harper, Columbia University; Professor R. G. Harrison, Yale University; Professor G. A. Hulett, Princeton University; Professor W. Lindgren, Massachusetts Institute of Technology; and Professor E. W. Brown, Yale University, chairman. This subcommittee has conducted its discussion mainly by correspondence, but held a meeting on May 14 last at which four of the members were present. The report which follows is signed by these four members of the sub-committee; Professor Harper, being absent from the meeting and being unwilling to sign the report, resigned.

The education of students naturally is divided into school, undergraduate and postgraduate instruction. The first of these is too large a question to touch on in this connection. The third on the whole is well organized, and at the present time practically consists only of students intending to take up research or those needing the higher degrees for educational careers. Hence most of the work of the sub-committee was in the direction of considering what might be done to further the interests of the abler students in their undergraduate careers. In using the phrase "abler students" the subcommittee had in mind the upper twenty to thirty per cent. of the classes.

In order to find out what was done in the various colleges and universities of the United States a circular was sent out to about forty, which were considered representative of the various systems of instruction throughout the country. This circular inquired what facilities were provided for the more able amongst the undergraduates for furnishing them with a better and more rapid training than the ordinary student. If such facilities were offered, inquiry was made as to what form these facilities took. It was also asked whether any money rewards were given for high attainments, and whether the institution had any knowledge of the results obtained from these facilities. About thirty of the forty selected institutions sent replies sufficiently detailed for the subcommittee to obtain a fair idea of what was being done throughout the country in this connection. It appears that five universities have specially organized courses in which the better students are able to have greater facilities for progress than the majority. Two others have courses laid out without, however, appearing to put much stress upon them. The remainder have little or nothing organized. These organized courses are generally referred to as "honors courses." Practically all of the institutions adopting them use the same methods, consisting of segregation into separate classes, extra work in connection with the ordinary courses, a limit for admission to such courses, a general final examination, less class-room work, and a complete program for junior and senior years. These different methods depended on the particular department, much freedom being given to the various departments. A fair idea of the various plans adopted may be gathered from the catalogues of the universities of Columbia, Minnesota, Princeton and Yale. Princeton has the preceptorial system in addition, but the expense of this makes it of doubtful value for

most institutions and it is not considered essential.

The subcommittee approves in general of these methods of looking after the interests of the abler students and strongly recommends that equivalent plans be made by all institutions of collegiate rank. As stated above, these methods refer to the work in junior and senior years. In freshman and sophomore years, where many classes are large enough to be taken in sections, it is recommended that the sections be formed according to the ability of the students as early as this can be ascertained. In this manner the best students will be able to advance more quickly and so be better prepared for the special arrangements for the later years. The subcommittee feels that in the past nearly all the time and energy of the instructor has been given to the lower end of the class, and that in consequence, the upper end has seriously suffered. While it may be true that a certain amount of stimulus is given to the whole class by the best men, the loss to the latter far outweighs any possible gain that might accrue to the majority from the presence of these men in the classroom. The latter often leave college with diminished powers if increased knowledge, having never felt the need of making great efforts in order to accomplish what is asked of them. The result is a serious loss of time and efficiency even for those who later take up professions which demand hard and concentrated work. The loss to the community is even more serious, particularly in respect of the number of men who will not make new efforts to develop their full capacities. The real interests of the nation are better served by giving to the upper twenty per cent. of the class an education suited to their abilities than by allowing these abilities to be frittered away for the sake of a doubtful gain to the remaining eighty per cent. In any case, it is not likely that the education of the latter will suffer under the democratic principles of our government.

As to the methods to be adopted, the subcommittee favors segregation into separate classes as in general the best. The question, however, of expense to the institution arises. This can be met in several ways. In most departments elective courses are given which are taken only by a few students. The majority of these are naturally the best students in the department, but there are generally a certain number who take an elective for other reasons. It is recommended that the latter be excluded unless their previous work is of high grade so that these courses may be confined to the best students. It will be found in most cases that a considerable number of the elective courses can be so confined without serious loss to the rest of the students. Where this plan can be adopted no new expenditure for extra courses will be incurred. The excluded students will choose from the remainder of the list of electives, which, in the larger institutions at least, is greater than the needs of those who are not making a serious study of the subject demand.

In some subjects where segregation is not feasible extra work can be assigned, but this work should be carefully laid out and published in the catalogue as an essential part of the honors course and not left to the momentary inclination of the instructor.

Many institutions report that their instructors give extra time to promising students. While in isolated cases much benefit is derived by the students, it is not recommended as a permanent plan. Additional work for which he receives no compensation is laid on the instructor already overburdened with teaching, and an incentive from competition to most of the better students is lacking.

Harvard University reports that it has no specially organized plans for the interests of the abler students, but that many of them are able to take advanced courses in their undergraduate years because there is no sharp line of distinction between graduate and undergraduate courses. This method has been adopted for several years in some of the larger institutions with fully organized graduate departments, but it is not possible for the great majority of the colleges.

A final examination covering the whole of the special work for the honors course is advisable, and a considerable amount of stress

should be laid on the results. The subcommittee recommends that the question of outside examiners for this purpose be seriously considered. These examiners would set the papers but not necessarily examine the answers of the students. It was pointed out that with the system of outside examiners there is a considerable degree of cooperation between the student and his instructor. The papers should deal mainly with the general principles and fundamental facts of the subject, and only rarely with questions of detail in special portions of the subject.

The subcommittee also believes that the courses might very properly include one or two subjects in each department for which no class-room instruction is provided.

It is believed that the effect of these methods will be greater progress and more thorough instruction to the better students in the fundamentals of all subjects. A higher degree of stimulation to work and to take an interest in scientific pursuits will undoubtedly result, so that greater numbers with better training will be induced to enter the graduate schools and to take up research as a permanent career. These students will also feel that their work is of value to them and will not search outside for opportunities to make use of their powers.

The subcommittee obtained reports on the systems adopted in Germany, France and England. It did not appear that, at the present stage of American education, a close examination of the systems of Germany and France would be useful to the work of the committee. They are highly organized from the bottom to the top. In Germany and France admission to the university is gained by a difficult examination which eliminates the majority of those who can not obtain real profit from university courses. In German universities almost no pressure of any kind is brought to bear on any students good or bad, except by means of the final examination and the thesis. At Cambridge and Oxford the honors schools have long been definitely established. About forty per cent. of the students enter them and they receive separate instruction from the outset. Until the last two decades, but little thought was given to the work of the remaining sixty per cent., but the interests of the latter have recently been seriously considered and much improvement has been made.

A difficult situation is brought about by the student having no knowledge of his after career during his undergraduate course, so that he is not able to choose his line of work effectively. While vocational training is not in question in this connection, all the training for scientific work needs thorough and broad foundations. If the student has a knowledge of his after career he is able to choose his course so that these foundations may be well and truly laid. It is recommended, therefore, that advice be given to all students in college to choose their careers as early as possible.

The majority of those who expect to do research look to positions in colleges and universities in order to earn a living. Information should be given them as to research positions in the government service and also those in industrial laboratories. Work under the government appears to be somewhat better paid than that in the collegiate world, but there is some limitation on the problems which may be taken up for research. In the industrial laboratories these differences are still greater.

It is recommended that students be encouraged to change their institution between the undergraduate and postgraduate work. A greater breadth of view is desirable. It is also recommended that students be encouraged to choose their university for postgraduate work on account of the quality of the men in the subject rather than for other reasons. Much can be done by the advice of the undergraduate teaching body in this respect.

The subcommittee was only able to deal with these questions as far as the colleges and universities were concerned. Some information was forthcoming as to the situation in the technical schools, but it did not feel itself in a position to undertake this part of the work. It recommends that a new subcommittee consisting of a fuller representation of those connected with technical instruction be appointed to consider what measures should be taken to secure the objects in view.

THE NAVAL ADVISORY BOARD OF INVENTIONS

THE secretary of the navy has announced the membership of the Naval Advisory Board of Inventions which consists of twenty-three members, including Mr. Thomas A. Edison, who was selected by Mr. Daniels to serve as the presiding officer of the board. The other twenty-two members of the board who were chosen by ballot by eleven scientific societies invited by the secretary of the navy are:

American Chemical Society.—W. R. Whitney, Schenectady, N. Y. Massachusetts Institute of Technology, '90. Director of Research Laboratory of the General Electric Company. L. H. Baekeland, Yonkers, N. Y. University of Ghent, '82. In private chemical practise.

American Institute of Electrical Engineers.—Frank Julian Sprague, New York City. Naval Academy, '78. Consulting engineer for Sprague, Otis and General Electric Companies. B. G. Lamme, Pittsburgh. Ohio State, '88. Chief engineer of Westinghouse Electric and Manufacturing Company.

American Mathematical Society.—Robert Simpson Woodward, Washington, D. C. Michigan, '72. President of Carnegie Institution. Arthur Gordon Webster, Worcester, Mass. Harvard, '85. Professor of physics, Clark University.

American Society of Civil Engineers.—Andrew Murray Hunt, New York City. Naval Academy, '79. Consulting engineer. Alfred Craven, New York City. Naval Academy, '67. Chief engineer of public service commission and formerly division engineer in charge of construction work on Croton aqueduct and reservoirs.

American Aeronautical Society.—Matthew Bacon Sellers, Baltimore, Md. Lawrence Scientific School. Director of Technical Board of the Aeronautical Society of America. Hudson Maxim, Brooklyn, N. Y. Ordnance and explosive expert.

The Inventor's Guild.—Peter Cooper Hewitt, New York City. Inventor. Thomas Robbins, Stamford, Conn. Princeton. President of Robbins Conveying Belt Company.

American Society of Automobile Engineers.—Andrew L. Riker, Detroit. Vice-president of Locomobile Company. Electrical and mechanical engineer. Howard E. Coffin, Detroit. Michigan, '96. President of Hudson Motor Car Company.

American Institute of Mining Engineers.—William Laurence Saunders, New York City. Pennsylvania, '76. Chairman board of directors, Ingersoll-Rand Company. Benjamin Bowditch Thayer, New York City. Harvard, '85. President of Anaconda Copper Mining Company.

American Electro-Chemical Society.—Joseph William Richards, South Bethlehem, Pa. Lehigh, '86. Professor of electro-chemistry, Lehigh University. Lawrence Addicks, Chrome, N. J. Mass. Inst., '89. Consulting engineer for Phelps, Dodge & Co.

American Society of Mechanical Engineers.—William Leroy Emmet, Schenectady, N. Y. Naval Academy, '81. Engineer with the General Electric Company. Spencer Miller, South Orange, N. J. Worcester Polytechnic, '79. Inventor.

American Society of Aeronautic Engineers.—Henry Alexander Wise Wood, New York City. Engineer and manufacturer of printing. Elmer Ambrose Sperry, Chicago. Cornell, '76. Founder of Sperry Electric Company.

SCIENTIFIC NOTES AND NEWS

KARL EUGEN GUTHE, professor of physics in the University of Michigan and dean of the Graduate School, died on September 11, following a surgical operation. He was born in Hanover, Germany, on March 5, 1866.

JOHN HOWARD VAN AMRINGE, dean of Columbia College and professor of mathematics until his retirement five years ago after a service of fifty years, died on September 10, at the age of seventy-nine years.

THE Institution of Mining Engineers will present the institution medal for 1914–15 at its twenty-sixth annual general meeting, to be held at Leeds in September, to Dr. John Scott Haldane, F.R.S., of Oxford, in recognition of his investigations on mine air.

PROFESSOR JULIUS ELSTER and Professor Hans Geitel, who have carried on conjointly

experiments in physics and have published together over one hundred and twenty papers, have received honorary doctorates from the University of Göttingen.

Dr. Pircher has been made acting director of the Austrian Meteorological Bureau in succession of Professor Trabert, who has retired on account of his health.

Dr. Wilbur A. Sawyer, director of the State Hygienic Laboratory and lecturer in hygiene and preventive medicine in the University of California, has been appointed a member of the California State Board of Health and secretary of the board. As executive officer of the board he will have under his direction seven bureaus—Administration, Vital Statistics, Tuberculosis, and Registration of Nurses, with headquarters at the capitol, Sacramento; and the State Hygienic Laboratory and the Bureaus of Sanitary Engineering and of Food and Drugs, maintained by the state at the University of California.

Associate Professor Charles Riborg Mann, of the department of physics in the University of Chicago, has been granted by the university board of trustees an extension of his leave of absence for one year from October 1, in order that he may complete his survey of technical instruction in the United States, which he has undertaken under the auspices of the Carnegie Foundation for the Advancement of Teaching.

Mr. M. T. Dawe, lately director of agriculture, British East Africa, has been appointed agricultural adviser to the government of Colombia.

Professor Haendel, director of the Hygienic Institute in Saarbrücken, has been appointed director of the Imperial Health Bureau, Berlin, succeeding Professor Lentz.

Professor W. Morgan, who fills the chair of automobile engineering in the faculty of engineering of the University of Bristol, has been released from his duties for the period of the war to engage in work in connection with the production of munitions.

Surgeon Victor G. Heiser, U. S. Public Health Service, formerly commissioner of health, Philippine Islands, has arrived in New York. He has returned to the United States to make a report to the Rockefeller Foundation of his investigations in the Philippines and India of hookworm and other diseases.

Professor F. Cajori has resumed work at Colorado College after spending the past year abroad. He attended the Napier Tercentenary in Edinburgh and later traveled in France, Italy, Switzerland and Germany. The larger part of the year was spent in Oxford, Cambridge and London, where he was engaged in researches on the history of certain branches of mathematics in Great Britain during the seventeenth and eighteenth centuries.

Dr. E. S. Moore, professor of geology and mineralogy of Pennylvania State College, has returned from a year's leave of absence after attending the meeting of the British Association for the Advancement of Science in Australia and visiting several countries on geological excursions. The countries visited included New Zealand, India, Egypt and France, the last six months being spent chiefly in study with Professor Lacroix at Paris.

News has reached England of the Easter Island expedition of Mr. and Mrs. Scoresby Routledge up to June 8, at which date the expedition had been fourteen months in residence, during which time a careful survey had been made of the existing antiquities and such ethnographical information collected as is still available.

The twenty-fourth annual session of the Association of Military Surgeons of the United States was held in Washington on September 13 to 15, under the presidency of Col. J. R. Kean.

We learn from *Nature* that in addition to his name being expunged from the list of honorary members of the laryngological societies of Vienna and Berlin, in consequence of his having protested in a letter to the *Times* against the alleged barbarities of Germany in the war, the name of Sir Felix Semon has been removed from the *Internationales Centralblatt für Laryngologie,* which journal he founded twenty-five years ago. In consequence of this action, all the British editorial contributors to

the *Centralblatt* who have had an opportunity of seeing the declaration have withdrawn their names from and resigned their editorial connection with it. Among these are Dr. Peter McBride, Dr. H. J. Davis, Dr. Logan Turner and Dr. Watson-Williams. Their American collaborator, Dr. Emil Mayer, has also severed his connection with the journal.

A BRONZE bas-relief—the work of Mr. S. N. Babb—is about to be erected in St. Paul's Cathedral in memory of Captain Scott and his companions who perished in the Antarctic. At the request of the committee responsible for the memorial an inscription has been written by Lord Curzon, which reads as follows: "In memory of Captain Robert Falcon Scott, C.V.O., R.N., Dr. Edward Adrian Wilson, Captain Lawrence E. G. Oates, Lieut. Henry R. Bowers and Petty Officer Edgar Evans, who died on their return journey from the South Pole in February and March, 1912. Inflexible of purpose, steadfast in courage, resolute in endurance in the face of unparalleled misfortune. Their bodies are lost in the Antarctic ice. But the memory of their deeds is an everlasting monument."

DR. DONALD MCINTOSH, professor of veterinary science at the University of Illinois, died on September 5, at his summer home in Portland, Me. Dr. McIntosh was elected to his permanent position in June, 1886. At that time the total faculty of the university numbered but twenty-seven, of whom only Dr. Burrill, Professor Ricker, Professor Rolfe, Professor Baker and Professor Forbes are left.

THE death is announced at the age of eighty-eight years of Mr. F. Manson Bailey, colonial botanist for Queensland from 1881 until within a short time of his death.

DR. J. J. T. QUENSEL, professor of pathological anatomy at the University of Upsala, has died at the age of seventy years.

DR. RICHARD KIEPERT, the German cartographer, has died at the age of sixty-nine years.

CAPTAIN W. E. G. ATKINSON and Captain Arthur Kellas were killed at the Dardanelles on August 6. The former was known for his experimental work on varieties of wheat, the latter for work in psychiatry and physiology.

LORD BRABOURNE has been killed in the war in the twenty-ninth year of his age. He had returned recently from South America where he was collecting material for the work on "The Birds of South America" which he was writing in conjunction with Mr. Charles Chubb and of which one part had appeared.

THE Berlin correspondent of the *Journal* of the American Medical Association writes that the "Langenbeck-Virchow Haus," built by and for the Berlin Medical Society and the German Surgical Association was opened on August 1. The ceremonial opening was postponed until after the conclusion of the war. The auditorium, three stories high, has a seating capacity of 900. The galleries have a seating capacity of 335. The room is lighted by day through a skylight and in the evening by eighteen electric arc lamps of 25,000 candle power. The auditorium is 13 meters high, 24 meters long and 17.5 meters wide. Artificial ventilation is provided for so that the air may be renewed every hour. On the first floor is a smaller hall with a seating capacity of 200. Both rooms are provided with epidiascopes and kinetoscopes and can rapidly be darkened. Small rooms, contiguous to the auditorium, are provided for waiting rooms for patients, and in one a small laboratory has been installed. The reading room and library, containing 200,000 volumes, is on the third floor. One small room contains the library bequeathed to the Berlin Medical Society by Virchow. The larger reading room is furnished with twenty-five tables at each of which two may be seated. Other small rooms are provided for such readers as wish to work quietly and undisturbed. Refreshments may be had on the first floor. Stores occupy the first floor front, and will be rented to concerns identified with medicine, such as instrument houses, book dealers, etc.

UNIVERSITY AND EDUCATIONAL NEWS

GUY'S Hospital has received $125,000 from the trustees of the will of the late Sir William Dunn for the endowment of a lectureship in pathology in the Guy's Hospital Medical School, to be known as the "Sir William Dunn Lectureship in Pathology."

The registration for the fall term at the University of California by September 3 had reached a total of 5,551, as compared with 5,236 on a corresponding date in 1914. Graduate students at that date numbered 742 as compared with 632 on a corresponding date the previous year. Including the summer session of 1915, which enrolled more than 5,400, and the students in the colleges of medicine, dentistry, pharmacy and law, but excluding students of the university farm school, the university extension division, the Wilmerding Trades School, and of the San Francisco Institute of Art, the University of California's registration for the present academic year is expected to exceed 11,000.

The department of sociology and anthropology, University of Minnesota, has been reorganized with Dr. Albert Ernest Jenks as chairman; Dr. Arthur J. Todd, professor of sociology; Dr. Paul I. Neergaard, instructor in sociology; Mr. Frank J. Bruno, lecturer on poverty; Mr. Otto W. Davis, lecturer on housing; Mr. Charles C. Stillman, lecturer on poverty. Dr. Jenks has been professor of anthropology in the University of Minnesota for nine years. All the other members of the department are new men in the university. Dr. Todd comes from a professorship of sociology in the University of Pittsburgh; Dr. Neergaard was last year instructor in sociology at Western Reserve; Mr. Bruno is secretary of the Minneapolis Associated Charities; Mr. Davis is housing expert with the Minneapolis Civics and Commerce Association; and Mr. Stillman is secretary of the United Charities of St. Paul. The president of the university, Dr. George E. Vincent, will contribute a course of lectures on "Aspects of Social Psychology." Dr. Joseph Peterson, another new member of the faculty, and professor of psychology, offers a semester course of lectures on social psychology for the department of sociology and anthropology. Another new course of lectures will be presented by experts in collaboration from the several detention institutions of the state. It is the plan of the department to emphasize practical courses to equip the students for life in the extensive rural states which stretch westward with Minneapolis as their gateway.

Dr. Chas. H. Otis, for the past two years instructor in botany in the College of Arts and Sciences of Cornell University, has accepted a position in the botanical department and experiment station of the New Hampshire College.

Dr. Anschultz, docent in the Hamburg Scientific Institute, and Dr. Demoll, professor of zoology in the Karlsruhe Technical School, have accepted calls to professorships in the university at Constantinople, the former in psychology and the latter in zoology.

DISCUSSION AND CORRESPONDENCE

CALIFORNIA AND STANFORD MISREPRESENTED

In the Ninth Annual Report of the Carnegie Foundation for the Advancement of Teaching which has just come to my attention, "the two great universities of California" are accused of having lent themselves "to the perpetuation of the medical rivalry which has so long existed in San Francisco." Mr. Pritchett rightly adds, "The world has a right to expect a better solution than this and one more in accordance with the largeness of true university relationship." The solution referred to by Mr. Pritchett is the question of fusion of the two university medical schools.

Were such a grievous charge against our universities justified, every right thinking man would agree with Mr. Pritchett that this *is,* not merely *seems,* "a matter of regret from every point of view." Since this charge has been given such wide circulation and especially since the distinguished board of trustees of the foundation, by virtue of their office, would seem to stand sponsors for Mr. Pritchett's sweeping indictment, I must record my earnest protest against so unfounded a charge. That any one animated solely by a desire to know and to understand the relations and aims of our universities could so wholly misunderstand and misrepresent them, is as regrettable as it is surprising. It is perplexing, indeed, to imagine where Mr. Pritchett found evidence to prompt such a serious reflection upon the good name of California and Stanford.

I have been in California only six years—happy years—but these six years more than cover the period during which the fusion of the two university medical schools has been under consideration. During this period the universities have not lent themselves to so unworthy a purpose. The institutional relations have been friendly, indeed, and a spirit of cooperation has prevailed throughout. This is in keeping with the spirit of the west. The disregard for little things, the helping hand and feeling heart, are the legacy of pioneer days just passed. Besides, there really is very little occasion or basis for unseemly inter-university rivalry. Stanford set its limits regarding enrollment and is maintaining them, and with an attendance of 7,000 *our* state university surely is not lacking in numbers. Every year some of our medical students are advised to attend the California summer school, not only in the non-medical, but in the medical subjects as well. We accept each other's records without hesitation or question and also encourage students who desire to do so to go elsewhere. We have trusted each other and the rewards of this trust have, I believe, been ours. The spirit of reciprocity prevails. We Stanford men were not all "to the manner born" but we are citizens of California and as such have faithfully espoused the best interests of our state university. More than a score of us are alumni of California, whose faculty also contains a number of Stanford graduates. Besides, many members of the faculties of the neighboring universities have a common alma mater. Larger appropriations and opportunities for California neither alarm nor threaten us. If we have not decided to merge the medical, law or engineering schools or even our universities, that is no reason why our motives should be impugned. Moreover, to my knowledge the faculty of Stanford University has never even considered such a fusion and the University of California must in this matter speak for itself. The subject, to be sure, has been considered in the administrative boards and may, I presume, be considered again, for I believe that the same good will animates them.

It is strange, indeed, how Mr. Pritchett can call our universities "great" and our medical schools "strong" if the alleged spirit prevails, for that way, surely, only weakness lies. Mr. Pritchett's characterization of the field of modern medicine as "*so narrow*" is decidedly enlightening. Other statements in Mr. Pritchett's report call for comment but I shall forbear. The future will be Mr. Pritchett's and our sternest judge. I trust, however, that a sense of justice will cause Mr. Pritchett to give an explanation for his unqualified accusation, and since the great usefulness and influence of the foundation must in time be seriously jeopardized by such uncorrected errors, I further trust that the board of trustees of the foundation will disclaim responsibility for so serious and so unjust a reflection upon the good name of the two universities.

"Those principles of peace and conciliation which President Jordan has so eminently represented" are indeed being maintained between the two universities, and if I may reciprocate Mr. Pritchett's wish, I hope that the same principles of peace and conciliation which Mr. Carnegie has so long and so ardently espoused will more and more pervade the spirit and temper of the verdicts of the Foundation for the Advancement of Teaching.

A. W. MEYER

PALO ALTO, CALIFORNIA,
August 4, 1915

SUCCESSFUL LONG-DISTANCE SHIPMENT OF CITRUS POLLEN

IN connection with investigations in Japan in the spring of 1915, Mr. Walter T. Swingle, physiologist in charge of crop physiology and breeding investigations, Bureau of Plant Industry, found it desirable to make an attempt to breed canker-resistant[1] strains of grapefruits and tangelos by hybridizing with the more resistant Japanese races of pumelo (Buntan) and other late-ripening, large-fruited citrous fruits commonly grown in Japan. He accordingly cabled for grapefruit and tangelo pollen.

[1] Hasse, Clara H., "*Pseudomonas citri*, the Cause of Citrus Canker," *Jour. Agric. Research*, Vol. 4, pp. 97–100, Pls. 9, 10, April, 1915.

Previous experiments had shown that it was possible to use pollen from flower buds which had been gathered when just ready to open and kept in cold storage until needed, but after five to seven days the buds discolored and moulded. Pollen had been sent in this way from Florida to California, but for a long period of time such as the duration of the voyage from Florida to Japan, it was necessary to develop other methods.

The attempt was first made to brush the pollen[2] from the anthers into small vials, but this process was abandoned for the much quicker method of putting the anthers entire into the vials. The preparation of the pollen may be divided into four methods, as follows: I., pollen in cork-stoppered vial; II., anthers in vial with cotton stopper; III., anthers in vacuum glass tubes, *i. e.,* tube filled with anthers for 1–2 inches, cotton ½ inch, then exhausted to about 10 mm. pressure and sealed; IV., anthers in dried vacuum glass tubes, *i. e.,* tube filled with anthers 1–2 inches, cotton ½ inch, exhausted to about .5 mm. pressure in the presence of sulfuric acid, the tube then sealed. As far as practicable the pollen was kept at a temperature of 10° C. until sealed.

Through the courtesy of Director Onda of the Imperial Horticultural Experiment Station at Okitsu, Shidzuoka Ken, Mr. Swingle made arrangements to test the viability of the pollen as well as to make hybrids in the variety collection of citrous fruits. Professor Y. Kumagai, of this station, kindly agreed to test the viability of this pollen in 30 per cent. cane sugar solution. His careful observations show conclusively that pollen can be successfully shipped from Florida to Japan and be in viable condition on arrival, four to six weeks after it is gathered.

Grapefruit pollen collected April 6, from one sealed tube (method III.) which was opened May 17, 1915, showed within forty-eight hours a germination of 50 per cent. with the pollen tubes fifteen times the diameter of the pollen grain. Fresh Joppa orange pollen used as a check showed the same germination (50 per cent.) within twenty-four hours, with pollen tubes twenty times the diameter of the pollen grains. Fresh "Ogasawara grapefruit" used as a check showed 80 per cent. germination inside of twenty-four hours, with the pollen tubes twenty times the diameter of the pollen grains. Pollen of Valencia Late oranges used as a check showed a germination of only 20 per cent. within forty-eight hours, and a length of pollen tube of but three to four times the diameter of the pollen grain. Other tubes of grapefruit and tangelo pollen prepared in the same manner (*i. e.,* method III.) showed from 2 to 10 per cent. germination, both with pollen tubes from two to five times the diameter of the pollen grain, while still others gave no results whatever. From observations upon these different lots of pollen it is probable that this may have been due to the pollen having low vitality when gathered. It is obvious, also, that there may be a variation in the viability of pollen of different varieties, or even in pollen from individual flowers.

Grapefruit pollen sent by methods I. and II. showed 7–8 per cent. germination within forty-eight hours, and pollen tubes ten times the diameter of the grain.

Pollen prepared by method IV. was sent late in April, so that no report has as yet been received showing the percentage of germination. However, in a cablegram sent from Tokyo July 8, 1915, Mr. Swingle reports: "Dry pollen successful," indicating that the most promising method for shipment of pollen over long distances is the one last noted, of drying in vacuo over sulfuric acid.

The necessity for stricter quarantine regulations to exclude dangerous diseases and insect pests already operates to prevent the free shipment of many plants from one country to another. As such regulations become more strict, the difficulties of securing plants increase. It is likely, however, that in most cases pollen shipped in vacuum tubes could be

[2] The sources of pollen were Bowen grapefruit and tangelo twigs bearing flowers fully matured but not yet open, gathered at Eustis and San Mateo, Florida. The lower part of each bundle of stems was packed in moist sphagnum, the bundle then wrapped in oiled paper and mailed from Florida to Washington in ordinary mailing cartons.

sent without danger of carrying plant diseases or insect pests.

MAUDE KELLERMAN
BUREAU OF PLANT INDUSTRY

VALLEY-FILL OF ARID INTERMONT PLAINS

UNFAILING tendency too broadly to generalize from a new-found principle is nowhere better shown than in the instance of ascribed origin of the wide intermont plains of the Great Basin in particular and in general of all desert tracts of the globe. So graphic are the descriptions of Basin Range features given by the various members of the famous Fortieth Parallel Survey that even after the elapse of half a century they continue to hold first place with scarcely a question concerning the accuracy of their genetic foundation.

One statement of the late Professor I. C. Russell furnishes the keynote to the whole problem. He speaks of the mountains of Nevada being "buried up to their shoulders in the débris of their own substances." As a corollary he ascribes enormous depths of 2,000 to 3,000 feet to the valley-fill between the various basin ranges. Russell's observations, as well as those of others, are mainly impressions gained on hurried reconnaissances through the region; and the statements made at the time really had little to substantiate them. The conceptions which they represent are in the extreme brilliant and suggestive. For this very reason it is that they go so long unchallenged.

Singularly enough one of Russell's most typical examples of buried mountains, and one oftenest cited as around which the valley-fill is thickest, is a district wherein subsequent investigation conclusively shows the valleys or intermont plains to have rock-floors. In these valleys the strata of the bed-rock are flexed and tilted often to a vertical attitude. The planed surface coincides nearly with the present ground surface. The wash or valley-fill is almost nil. To be sure there may be some instances in which there is a valley filling that has greater or less depth; but in many cases the broad intermont basin has a very pronounced rock-floor and the thickness of regolith or soil mantle is inappreciable.

Other critical data now exist that bear directly upon the extent of the valley-filling. The larger number of deep drill-holes, which have been put down in the desert regions of the west during recent years, furnish some very conclusive evidence touching the points under consideration. Of course well-logs, as a rule, are notoriously fanciful and, without proper checks, can not be implicitly relied upon. Yet many such records are adduced as proving the great depth of valley-fillings.

In a number of cases, which are really test-cases, depths of 2,000 to 3,000 feet are reported as being entirely in wash material. These statements are even presented in scientific literature. In one instance, in which soft Eocene clays and sands were dipping at an angle of 70 degrees, the drill is reported as having penetrated nearly 2,500 feet of wash débris without passing through it. In another case, that of the Santa Cruz Valley, near Tucson, Arizona, the valley-fill was said to be over 2,000 feet thick as shown by the drill; yet the late W J McGee found bed-rock near-by covered only by a few inches of soil.

One of the latest cases of this kind is the interpretation of deep-drill records in the Hueco (Tularosa) bolson in southern New Mexico. Drill-logs of more than 2,000 feet are given as evidence in support of the contention of the great depth of valley-fill. As a matter of fact, and as the records themselves clearly indicate, the beds passed through by the drill are the very red-beds that overlie the Carboniferous limestones of the region, and that one would expect first to encounter a short distance beneath the surface of the desert at those points. Abundant other data from this locality point rather conclusively to the fact that this so-called valley-fill is mainly not wash débris at all but typical soft red-beds. This seems to be another instance of forcing facts to fit theory.

What is still greatly needed in these desert investigations is further critical evidence bearing upon the geological date of the formation of the so-called Basin Range structures.

Until this is forthcoming from those travelers and explorers who are now working in this especial field the Basin Range hypothesis shall have to be considered as holding a place among those hypotheses yet unproven, and as an assumption of very doubtful utility.

Charles Keyes

SCIENTIFIC BOOKS

Mechanism, Life and Personality. By J. S. Haldane. New York, Dutton. 1914. Pp. viii + 139. Price, $1.00.

I

Dr. J. S. Haldane has long been known as a philosophical physiologist. Indeed it is now for more than three decades that he has occasionally relieved the labors of an orthodox and eminent scientific investigator with the pleasures of idealistic metaphysics. At length he has constructed his philosophy of biology into a little book, "Mechanism, Life and Personality," which he offers as a contribution towards "bringing the great biological movement of the nineteenth century into definite relation with the main stream of human thought."

The first half of this book is devoted to an examination of "the hypothesis that living organisms may be regarded as conscious or unconscious physical and chemical mechanisms, and can be satisfactorily investigated from this standpoint." Such is Haldane's statement of the mechanistic theory of life. Many considerations favor such a theory. Chemical analysis reveals no mysterious substances or reactions within the body, general physiology and the study of metabolism reveal no mysterious forms or manifestations of energy, and to all appearances the laws of the conservation of matter and the conservation of energy there hold. Consciousness, to be sure, is a difficulty, but, at any rate, consciousness seems not to interfere with the operation of any law of physics or of chemistry. Moreover, when once we have commenced the analysis of organisms, whether physically or chemically, we find no structure but physical and chemical structure, no activity but physical and chemical activity.

Historically too there is much to justify the mechanistic view, for "the history of physiology displays uninterrupted progress in the successful application of physical and chemical methods to physiological problems."

In the manifold and inconceivably intricate phenomena of organic regulations the mechanist has found serious difficulties. But in the course of time, as the mechanistic nature of nervous control, of the action of hormones, and of similar phenomena were discovered, this difficulty has grown less. Again the very existence of such marvellous physical and chemical structures as living things once seemed mechanistically quite inexplicable. But when Darwin conceived the principle of natural selection this difficulty was removed.

In his zeal to do full justice to the mechanistic theory Haldane even goes so far as to declare that it is possible to imagine how life may have originated. This is perhaps too much, for I suspect that some chemists would still prefer the first chapter of Genesis to the mechanist's guesses upon the subject.

As for the traditional opponents of the mechanistic view, the vitalists and the animists, their theories have ever been sterile. Occasionally encouraged by the collapse of one or another mechanistic theory, their own efforts have nevertheless ended in mere words, for "the apparent autonomous selective action of the organism turns out to be causally dependent in every detail on physical and chemical conditions." Therefore the action of any possible vital principle must be determined by these conditions.

Further the vitalistic theory implies "a definite breach in the fundamental law of the conservation of energy" (according to Driesch not in the first but in the second law of thermodynamics). Moreover the vitalistic agency is itself "entirely unintelligible."

On the other hand, even if the position of the vitalists and animists is entirely unsatisfactory, that does not establish the justice of the mechanistic theory. We must not forget that a living thing never does *seem* to be a mechanism, especially to those who know it well and study it as a whole, that is as a real *organism.* In particular to identify stimulus and response with physical and chemical causation, a belief

which is the very basis of the mechanistic physiology, is "a gigantic leap in the dark." To be sure, the difficulty of making out this causal connection might be due solely to the complexity of the cell, nevertheless "the point must be emphasized that in the case of stimulus and response there is in reality no experimental evidence whatsoever that the process can be understood as one of physical and chemical causation." No real quantitative relation between the supposed cause and the effect can be traced.

No doubt such information as we now possess will continue to increase, biophysics and biochemistry to unfold, but there is no reason to suppose that this kind of information will in the future serve as an explanation of that which in the past it has totally failed to explain.

Historically, in spite of the great services of physics and chemistry to biology, "the mechanistic theory has, on the whole, fared very badly." Cell-growth and cell-nutrition, absorption and secretion, have not been mechanistically explained. Mechanistic theories of respiration and metabolism, of muscular movement and other physiological movements, have also failed. And as the science develops we seem to get further and further away from any prospect of success in such enterprises. In truth ignorance alone could have justified the earlier crude mechanistic theories of the intracellular processes. For "what the mechanistic theory must assume in the case of an organism such as man is a vast assemblage of the most intricate and delicately adjusted cell-mechanisms, each mechanism being so constituted as to keep itself in working order year after year, and in exact coordination with the working of the millions of other cell-mechanisms which make up the whole organism."

But the facts of reproduction and heredity involve still greater difficulties, for we have reason to believe that the whole adult mechanism has come from the nuclear material of the fertilized germ cell. "On the mechanistic theory this nucleus must carry within its substance a mechanism which by reaction with the environment not only produces the millions of complex and delicately balanced mechanisms which constitute the adult organism, but provides for their orderly arrangement into tissues and organs, and for their orderly development in a certain perfectly specific manner." And yet, according to the mechanistic view, this structure of inconceivable complexity is capable of dividing itself to an indefinite extent while retaining its original structure. "The real difficulty for the mechanistic theory is that we are forced, on the one hand, to postulate that the germ-plasm is a mechanism of enormous complexity and definiteness, and, on the other, that this mechanism, in spite of its absolute definiteness and complexity, can divide and combine with other similar mechanisms, and can do so to an absolutely indefinite extent without alteration of its structure. On the one hand we have to postulate absolute definiteness of structure, and on the other absolute indefiniteness."

Hence, says Haldane, the mechanistic theory of heredity is impossible.

The mechanistic theory of heredity must involve in its downfall every other part of biology. "If we can not frame a mechanistic theory of heredity we are equally at a loss in connection with the ordinary phenomena of metabolism, and we have no right to use mechanistic hypotheses in connection with these phenomena." And finally Haldane concludes: "The phenomena of life are of such a nature that no physical or chemical explanation of them is remotely conceivable."

This conclusion leads to the second half of the book which begins with a philosophical discussion of the nature of reality. Out of this is developed the Hegelian conclusion "that a special category or categories ought to be added (to those of the physical sciences) for organic life, as the idea of life is one of the fundamental ideas. There is no reason why a category or general conception of life should not be just as much constitutive of our experience as the category of substance. Here, therefore, we have a possible way out of our difficulties with the mechanistic theory of life. In trying to reduce life to physical and chemical mechanism we are perhaps in some way con-

fusing two different categories. Kant's general philosophical conclusions have in any case thrown a quite new light on our conceptions of the physical world, and have taught us that the validity of these conceptions is of a very different nature from what was previously believed. It may be that just as we can not base physics on the purely mathematical conceptions of extension, so we can not base biology on the purely physical conceptions of matter and energy."

The whole living structure is organized, every part is definitely related to every other part. This is also true of its activity or metabolism. Thus it has come about that "in dealing with life we not only use a whole series of special terms, but these terms appear to belong to a specific general conception which is never made use of in the physical sciences." "The fundamental mistake of the mechanistic physiologists of the middle of last century was that they completely failed to realize this. Such processes as secretion, absorption, growth, nervous excitation, muscular contraction, were treated as if each was an isolable physical or chemical process, instead of being what it is, one side of a many-sided metabolic activity, of which the different sides are indissolubly associated."

"Our ordinary language as applied to life corresponds to these characteristics. We naturally speak of a living organism as an autonomous active whole, and think of it as such. The idea of its being a mechanism made up of separable parts, and actuated by external causes, is wholly unnatural to us, and becomes more and more unnatural the more we know about organisms."

"The concept we are using is radically different from any physical concept: for in conceiving what is living we do not separate between matter or structure and its activity."

"If we assume that the conception of the living organism is the fundamental conception of biology, it is clear that the aim of biology differs entirely from what it would be if the mechanistic theory were accepted. All attempts to trace the ultimate mechanism of life must be given up as meaningless."

On the contrary, the goal of biology must be the description of the organism as an organic unit. This proposition is illustrated by a discussion of the physiology of respiration, and the conclusion is reached that "the idea which gives unity and coherence to the whole of the physiology of respiration is that of the organic determination of the phenomena." And in general by means of this conception "we introduce order and intelligibility into biology, whereas there is no such order or intelligibility if the mechanistic theory of life be adopted."

Finally it is necessary to take account of one other characteristic of the higher organisms, of consciousness. Haldane's conclusion upon this point is as follows:

"We must, it seems to me, draw a sharp and clear distinction between biology, which deals simply with organic life, and psychology, which deals with conscious life or personality. This distinction is similar in general nature to that which I have already endeavored to draw between physics and biology. Just as biology is a more concrete science, nearer to reality than physics and chemistry, so psychology is a more concrete science than biology. We can abstract from the psychological aspect of a man or animal, and regard him only from the biological aspect. This is, in fact, what we do in physiology. In regard to most of the details of bodily activity there is little need for deliberate abstraction, since the psychological element lies only in the background. But when we come to deal with the bodily parts more immediately concerned in perception and voluntary response the case is very different. Perception, voluntary response, and conscious activity of every kind belong to personality, and therefore can not as such be dealt with scientifically from the merely biological or physiological standpoint. We might as well attempt to establish physics on a basis which totally disregarded the facts on which the conceptions of mass and energy are based, as to establish psychology on a merely physiological basis."

"Physiology deals, and ought to deal, with living organisms just in so far as the observa-

tions relating to them can be ordered in terms of the conception of a living organism. Where, and in so far as, the conception of a mere organism fails, as in the facts relating to conscious activity, we must have recourse to another conception, that of personality."

"It is evident that in applying the conception of personality to man or animal we leave out of account the details of organic activity. But the details are there, and the only account we are in a position to give of them is in terms of the lower or less concrete conception of mere organic activity. If we go still further into detail we are reduced to a still more abstract account in terms of physics and chemistry. Hence although in giving an account of perception and volition as a whole we make use of the conception of personality, and can not otherwise state the facts, there is abundant room left for a physiological account of the sense organs, nervous system, muscular activity, etc., provided that we recognize that such an account always deals abstractly with the phenomena, for the sufficient reason that a fuller and more concrete account can not at present be given. In the same way we treat the action of the muscles on the limbs, or of the limbs on the environment, or of the environment on the sensory organs, from the merely physical standpoint. This is an abstract method of treatment, as we have already seen; but it is to some extent the only method available. Provided we do not make the mistake of confusing the physical account of the world with reality, we are perfectly justified in making all the use we can of this physical account."

II

It is no light task for a man of science to form a critical judgment of this book, for I believe that its weakness is on the philosophical side. Certain it is that there is great justice in Haldane's strictures upon the supporters of the mechanistic view. Not only have mechanistic theories of physiological actions been almost uniformly of a childish crudeness, falling far beneath the complexity of the facts, but the mechanists have indeed, in the past, failed to recognize the significance of organization. And for my part I think that Haldane is quite right in establishing organization as something of a different order from mechanism, and elevating it into a category. The mechanists, having been obliged to isolate the phenomena, because such is the necessary condition for the physical and chemical study of them, have forgotten what they have done, and have not thought about organization at all.

It is, however, one thing to recognize the weakness of particular mechanistic theories of the past, or the difficulty, or even the inconceivability, of a mechanistic theory of heredity, and it is quite another thing to conclude that such a theory is impossible, especially in the face of Morgan's recent researches. The explanation of that which Darwin explained was once inconceivable. And one wonders what Galileo or Newton would have done with an electric battery if he had been asked to explain it as a mechanism. It is quite true that we possess no clue to the mechanism of the cell in general as distinguished from important particulars; it is perhaps probable that the task is too great for the human mind, but it is not possible by such a discussion as Haldane has given in the first part of his book to prove its ideal impossibility. The cell is a contrivance unlike anything which we understand, but so for Newton would have been an electric battery, and without further information he simply could not have begun to think about it.

When we turn to Haldane's philosophical objections to the mechanistic standpoint we encounter, as I believe, grave inconsistencies in his argument. True it is that "we can not base physics" *exclusively* "on the purely mathematical conceptions of extension," but physics would be in a very bad way indeed in an ungeometrical universe, or if it were obliged to get on without geometry. Geometry has no need of physics, it is true, though Archimedes showed how to solve geometrical problems by means of mechanics, but physics has imperative need of geometry. Geometry knows neither mass nor energy, but physics knows and uses points and lines.

In exactly like manner physical science has no need of the idea of organization, and knows it not. But biology, with organization as its central fact, both knows and uses physics and chemistry. Logically the less abstract encloses and includes the more abstract. The more abstract meantime preserves its full validity in the domain of the less abstract, just as, for example, the laws of number and extension hold in the physical sciences. So generally true is this that there is hardly any need of seeking illustrations. Haldane's own studies are studies of the organization of the physical and chemical processes of respiration. There can be no doubt that the idea of organization is what informs and interprets such investigations, and that it is an indispensable aid in their pursuit. Quite recently, for example, it has successfully guided Cannon in his researches on the physiology of fear and rage.

There is even a possibility, we may note in passing, in a certain restricted field, of pursuing the study of organization without regard to physics and chemistry. But that field is quite different from physiology, it is the field of animal behavior. In physiology there is no such possibility.

The truth seems to be that the relation of an organism to cellular mechanisms is not unlike the relation of a symphony to the sound waves which bear it to the ear. It is absurd to regard the symphony as merely the sum of the waves of sound, just as it is absurd to regard the organism as merely the sum of the biophysical and biochemical phenomena. But it is quite as absurd to deny that the sound waves are in a very real sense (even if they are not in "reality") the component parts of the symphony. They are, moreover, the only component parts which at present can be profitably investigated, as the difference between the substantial character of musical science, and our vague ideas about the individuality of thematic material well shows. If we turn to Haldane's own experimental researches we shall find that that is precisely his own standpoint as a practical physiologist; he analyzes the phenomena of organization into their component physical and chemical parts. If then "all attempts to trace the ultimate mechanism of life must be given up as meaningless," that can be only because there are only mechanisms, no *ultimate* mechanism of life. And for my own part I am obliged to say regarding his statement: "The phenomena of life are of such a nature that no physical or chemical explanation of them is remotely conceivable," that it is true only in a sense quite different from its apparent meaning, and is of no *scientific* interest.

A sound understanding of the relation between organic unity and physical phenomena involves no hypothesis regarding the nature of the external world or of reality. It may in the past have had a tendency to involve false ideas upon that subject in much the same way that the practical life of affairs does. But in physiology as in physics there is, I believe, no need to worry about the nature of reality. If the physiologist has foolish or mistaken notions on that subject, it is his private concern. Such ideas may affect his attitude toward the world; they do not affect his attitude toward his science. For in that he is dealing not with "reality," but with "truth," and the "truth" of his physical and chemical discoveries, when properly attested, is of exactly the same order as the truth of a proposition in geometry or of a law of harmony, which is enough.

Another characteristic of Haldane's thought is that he seems to attribute more value to concrete than to abstract scientific knowledge. From the purely metaphysical point of view such an attitude is quite intelligible. But scientifically it appears to be a matter of taste. The mathematical law will always have its devotees, and it will be many a day before such men will see in the progress of psychology anything to equal Newton's "Principia" in interest, in value or in greatness. And yet I am persuaded that such men will heartily recognize the concept of organization for what it is. They must then admit the need of Haldane's most interesting and timely discussion of a very difficult subject, and repay him with their gratitude.

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The Electron Theory of Matter. By O. W. RICHARDSON, Wheatstone Professor of Physics at Kings College, London. Pp. vi + 612. Cambridge Univ. Press. 1914.

THIS is in many ways a very remarkable book. Its scope is broader than that of any book on Electron Theory which has yet appeared, and it has the unique merit of not following even remotely the outline of J. J. Thomson's epoch-making work in this field. The author himself has exhibited within the past fifteen years, an unusual combination of theoretical and experimental fertility, and the present volume represents his digest, from the beginning, of the whole field of electromagnetic theory from both the theoretical and the experimental side. It exhibits profundity of scholarship, breadth of knowledge, enormous industry and a commendable fairness and reasonableness of temper.

The first 216 pages contain mainly the author's own treatment of nearly all of the most important of the classical theorems of electromagnetism such as the various potential theorems and those growing out of the Maxwell equations. From this point on is found a very exhaustive and original treatment of practically all of the newer developments of physics the scope of which can best be seen from the chapter headings. There are eighty pages on the electrodynamics of a moving charge, including a full discussion of the Abraham and Lorenz theorems; sixty pages on relativity; thirty-five on radiation and temperature with Wien's and Planck's contributions; forty on the theory of magnetism with a full review of Weiss' work; seventy-five pages on the electron theory of metallic conduction, thermo electromotive force, and thermoionics; thirty-five pages on "Types of Radiation" corpuscular and ethereal, including recent X-ray theory; thirty-five pages on spectroscopic phenomena; forty on the structure of the atom with Thomson rather overdone and Nicholson and Bohr somewhat slighted; and sixteen on gravitation.

Altogether it is a book of large and permanent value and another testimony to the breadth and fecundity of British science.

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SPECIAL ARTICLES

A SYSTEM OF RECORDING TYPES OF MATING IN EXPERIMENTAL BREEDING OPERATIONS[1]

ALL Mendelian experimentation with bisexual forms implies a system of mating which in practical work is called line breeding. One starts any Mendelian experiment with two kinds of organisms which are crossed with each other to produce the F_1 generation. Then the F_1 individuals are either mated *inter se* or back-crossed to the parent forms. The F_2 individuals may be mated in a variety of ways *inter se* and with the parents or grandparents.

Many of those engaged in Mendelian work

Diagram I

P generation BPR ♂ × Houdan ♀

F_1 generation F_1 ♂ × F_1 ♀ from reciprocal cross

F_2 generation F_2 ♂ × BPR ♀

F_3 generation F_3 ♂ *M*

Diagram II

P generation Houdan ♂ × BPR ♀

F_1 generation F_1 ♂ × Houdan ♀

F_2 generation F_2 ♂ × F_1 ♀ from reciprocal cross

F_3 generation F_3 ♀ *N*

must have experienced the same difficulty that the writer has in recording experimental results, namely, that of expressing adequately and completely, and at the same time briefly and simply the general nature or type of the

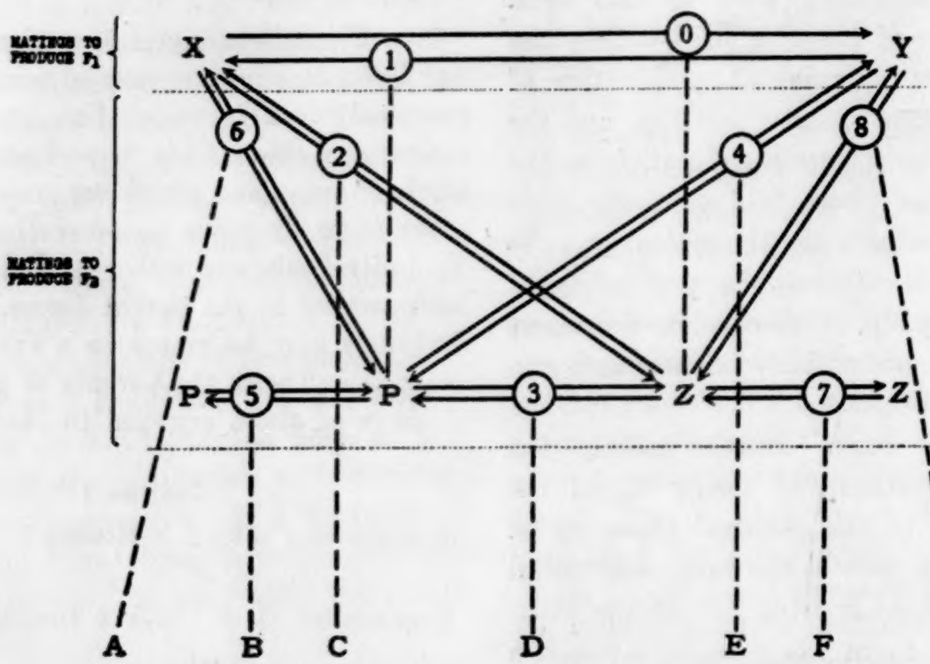
1 Papers from the Biological Laboratory of the Maine Agricultural Experiment Station, No. 88.

pedigree by which particular individuals in the F_2 and F_3 generations are descended. To illustrate the meaning here let us consider the two individual fowls *M* and *N* produced as indicated in pedigree diagrams I. and II.

To describe in words how *M* or *N* was bred is a tedious piece of business. They are both F_3 individuals from a cross of the same two breeds of poultry, Barred Plymouth Rock and Houdan. Yet their breeding is very different. It is of the utmost importance in planning breeding experiments, especially when one comes to the matings of F_2 individuals, to have a clear picture in one's mind of the breeding of each individual back to the original cross. The writer has wrestled for some time with this problem and tried out various schemes, such as the use of initial letters, figures for years, etc. None of these has proved satisfactory in practise. It finally seemed clear that the only entirely satisfactory solution (to the writer at least) would be one which was perfectly general. Such a general solution involves two things: first, a complete conspectus of all possible types of mating of the individuals of the P, F_1 and F_2 generations *inter se*, both within and outside their own generations, and second, a simple, pre-

TABLE I

Matings to Produce F_3



F_2 Individuals Mated	Number of Mating	F_2 Individuals Mated	Number of Mating	F_2 Individuals Mated	Number of Mating	F_2 Individuals Mated	Number of Mating
$A \times X$	10	$B \times Z$	46	$C \times F$	51	$E \times E$	19
$A \times Y$	12	$B \times B$	13	$C \times G$	53	$E \times F$	45
$A \times P$	40	$B \times C$	37	$D \times X$	22	$E \times G$	47
$A \times Z$	42	$B \times D$	29	$D \times Y$	24	$F \times X$	30
$A \times A$	11	$B \times E$	55	$D \times P$	52	$F \times Y$	32
$A \times B$	33	$B \times F$	57	$D \times Z$	54	$F \times P$	60
$A \times C$	25	$B \times G$	59	$D \times D$	17	$F \times Z$	62
$A \times D$	35	$C \times X$	18	$D \times E$	43	$F \times F$	21
$A \times E$	61	$C \times Y$	20	$D \times F$	31	$F \times G$	49
$A \times F$	63	$C \times P$	48	$D \times G$	27	$G \times X$	34
$A \times G$	65	$C \times Z$	50	$E \times X$	26	$G \times Y$	36
$B \times X$	14	$C \times C$	15	$E \times Y$	28	$G \times P$	64
$B \times Y$	16	$C \times D$	39	$E \times P$	56	$G \times Z$	66
$B \times P$	44	$C \times E$	41	$E \times Z$	58	$G \times G$	23

ferably numerical, designation of each one of these possible types, each such designation to be of course unique and permanent.

Table I. gives such a general solution of the problem of simply designating pedigree types, through the matings of F_2 to produce F_3. Beyond that it is not practical to go. A word should be said in explanation of the table. Letters denote *individuals* or groups of individuals which are brothers and sisters. Solid lines, with circles in their course, connecting letters, denote *matings* of the kinds of individuals indicated by the connected letters. Dotted lines lead from the mating to the kind of individual produced. Arrow heads indicate the direction of the mating, the arrow being supposed always to pass from the male to the female. Separate numbers are not given to reciprocal matings *after* the matings of the P generation to produce F_1. To designate separately reciprocal matings after that point would greatly complicate the system without any significant gain from a practical point of view. In the later generations, reciprocals may be indicated if desired, by affixing a sub-figure 1 to the designation of the mating.

The numbers within the circles are the designations (or names) of the matings, and from the very nature of the case, these numbers designate not alone the particular mating but also, in F_1 and later generations, the nature of the pedigree of each of the individuals entering that mating. This will be clear as we proceed.

This table is to be read in the following manner: Individual X♂ is mated with Y♀ to produce F_1 individuals Z, and this mating is designated 0. Individual Y♂ is mated with X♀ and produces F_1 individual P, and the mating is 1. The F_1 individuals, mated in all possible ways *inter se* and with the parents X and Y, as indicated in matings designated 2 to 8 inclusive, produce seven kinds[2] of F_2 individuals, A to G. These seven sorts of F_2 individuals bred in all possible ways *inter se* and with their parents and grandparents produce 56 sorts of F_3 individuals, as indicated in the lower half of Table I. As already noted, separate account of reciprocals is not taken.

[2] "Kinds" referring here only to the manner in which the individuals have been bred.

The use of the table may be indicated by some examples. Suppose one wishes to mate in an experiment the two birds called M and N in an earlier paragraph of this paper. He will wish to indicate in some way in his notes the previous breeding history of each of these birds. If he does this verbally—and hitherto this appears to have been the only way of reaching such end—he must say of individual N, for example, something like the following: "This F_3 bird was produced by the mating of an F_2 male with an F_1 female produced by mating a Barred Plymouth Rock male with a Houdan female. The F_2 male was himself produced by the mating of an F_1 male, out of the cross Houdan male by Barred Rock female, on a pure Houdan female." Quite apart from the amount of space involved in such a setting forth of the facts, it is very difficult to form quickly a clear mental picture of the bird's pedigree from this tedious verbal exposition. Suppose we are using the system of pedigree designation discussed in this paper we could then cover all the facts set forth above about bird N by merely writing in our notes

"Bird N (58) BPR-Houdan series,"

and to describe completely the mating of M with N we have merely to write

"F_3♂M (22) × F_3♀N (58) BPR-Houdan series."

The figure 58 in the case of N means that she was produced from a mating of the type indicated in the table as $E \times Z$, and the des-

DIAGRAM II

Houdan ♂ × *BPR* ♀
(Y) (1) (X)

F_1♂ × Houdan ♀
(P) (4) (Y)

F_2♂ × F_1♀ from reciprocal cross
(E) (58) (Z)

N

ignation of such a mating is 58. This will be made clear by repeating the pedigree diagram of bird *N, Diagram II.*, and adding to it the proper letters and mating designations from Table I.

The simplicity of the scheme is obvious. No argument appears necessary as to its usefulness in experimental breeding operations. The writer has found it extremely helpful and clarifying.

A word should be added in regard to the system by which the numbers have been assigned to the matings. It might at first sight appear as though the arrangement were an entirely haphazard one. It is not. On the contrary the numbers will be found to conform to the following general principles, which seem likely to be of aid in practical work, as tending to make it easy to recall from a number just what its particular pedigree looks like.

1. All even numbers refer to back-cross matings.

2. All odd numbers refer to co-fraternal or intra-generation matings (not back-crosses).

3. Matings below 2 are of parental generation individuals: between 2 and 8 inclusive are of F_1 individuals; matings over 10 are of F_2 individuals.

4. Even numbers from 10 to 36 inclusive designate back-crosses of F_2 individuals with their *grandparents,* or individuals of the grandparental generation.

5. Even numbers from 40 up designate back-crosses of F_2 individuals on F_1 individuals.

6. In the case of the odd numbers from 11 up it is, *in a general way,* true that the smaller the designating number of a mating the more closely related to each other are the two individuals entering that mating likely to be. This principle of assigning the numbers could not be so precisely followed as the preceding five, but still is perhaps worth a little.

In using this system in one's notes or writing it is of course essential to have the basic table always at hand. If the plan should appeal to any number of experimental workers it would be a simple matter to have copies of Table I. printed on heavy cardboard to be used in breeding houses and pens, in the field and at the desk.

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THE CHEMICAL COMPOSITION OF BORNITE

Since the analyses of crystallized material from Cornwall by Plattner,[1] bornite has generally been considered to be a cuprous sulfoferrite, $Cu_3FeS_3(3Cu_2S \cdot Fe_2S_3)$. In 1903 Harrington[2] made a critical study of the published analyses, added several new analyses, and concluded that the chemical formula of bornite is $Cu_5FeS_4(5Cu_2S \cdot Fe_2S_3)$. Recently Kraus and Goldsberry[3] made an analysis of crystallized bornite from Bristol, Conn., which gave the formula $Cu_{12}Fe_2S_9(6Cu_2S \cdot Fe_2S_3)$, and also confirmed Harrington's formula Cu_5FeS_4, of crystallized bornite from the same locality. They conclude that bornite is of variable chemical composition, and in order to explain the facts they assume a morphotropic series of minerals ranging from chalcopyrite, $CuFeS_2$, through barnhardtite, $Cu_4Fe_2S_5$, and various bornites $Cu_6Fe_2S_6$, $Cu_8Fe_2S_7$, $Cu_{10}Fe_2S_8$, $Cu_{12}Fe_2S_9$, up to $Cu_{76}Fe_2S_{41}$, finally ending with chalcocite Cu_2S, each member of the series differing from the one below it by the addition of one molecule of Cu_2S.

As a metallographic examination of the two analyzed bornites showed no foreign admixture, the work of Kraus and Goldsberry furnishes, for the first time, proof that bornite is variable in composition. It is believed, however, that there is a more rational explanation of the variability in composition of bornite than the one advanced by Kraus and Goldsberry.

The recorded analyses of bornite show a copper content varying from 77 to 55 per cent., and an iron content varying from 18 to 6 per cent. In Fig. 1 I have plotted on the triangular coordinate diagram of J. Willard Gibbs the available bornite analyses (59 in number) given in Hintze's "Handbuch" and in the

1 *Pogg. Ann.*, 47, 351, 1839.

2 *Amer. Jour. Sci.*, 16, 151, 1903.

3 *Amer. Jour. Sci.*, 37, 539, 1914.

articles of Harrington and Kraus and Goldsberry. (For analyses with more than two or three per cent. of gangue the percentages have been recalculated.) The small triangle furnishes a key to the larger diagram which represents one sixth the area enlarged tenfold. The diagonal line crossing the diagram is the locus of analyses of minerals in Kraus and Goldsberry's series, $Cu_xFe_2S_{\frac{x}{2}+3}$. This ranges from $CuFeS_2$ on the left to Cu_2S on the right. Most of the analyses are ranged along this line. Those much above the line are probably due to errors in the analyses or to the presence of oxidation products.

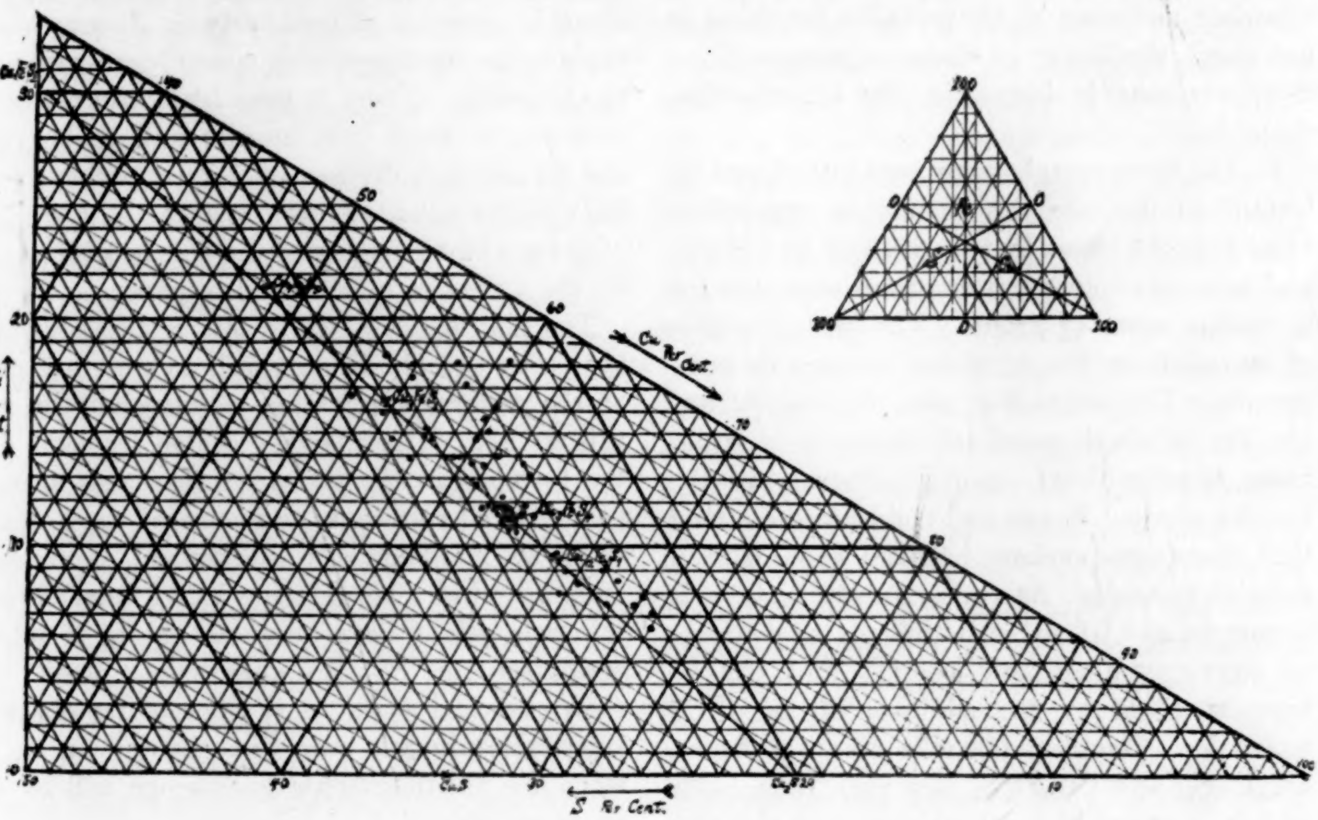

FIG. 1.

The only clustering of points in the diagram is around Cu_5FeS_4. Some may interpret this as evidence that bornite has the formula Cu_5FeS_4, but on the solid solution hypothesis advanced later on it may simply represent the average solubility. Most of these analyses were made upon massive material and as the study of polished sections proves, apparently pure, massive bornite usually contains small amounts of chalcopyrite, chalcocite or covellite, and occasionally other minerals. It is safe to say that very few of the massive bornites are entirely free from other minerals, but chalcopyrite and chalcocite, the two most common impurities in bornite, tend to neutralize the effect of each other for

$$CuFeS_2 + Cu_2S = Cu_3FeS_3$$

and

$$CuFeS_2 + 2Cu_2S = Cu_5FeS_4.$$

Because of this, and because the impurities are often trifling in amount, the analyses may be used with caution.

The lower limit of bornite seems to be represented by Cu_3FeS_3 with iron content of 16.36 per cent.; the Cornish crystals approach this formula and an artificial bornite made by Böcking[4] is very close to the theoretical for Cu_3FeS_3. Only seven analyses out of the fifty-nine show more than 16.3 per cent. iron. A slight admixture of chalcopyrite (Fe = 30.5 per cent.) will easily account for the excess iron. The lowest iron content on record is 6.4 per cent. The low iron content of some of the

[4] Hintze, "Handbuch der Mineralogie," Vol. I., p. 914, 1901.

bornites is doubtless due to admixed chalcocite just as the iron content of chalcocite is usually due to admixed bornite.

There are four possible explanations of the variability in chemical composition of bornite, viz: (1) Mechanical mixture, (2) isomorphous mixture, (3) morphotropic series (Kraus and Goldsberry), (4) solid solution.

1. While it is certain that mechanical mixtures explain part of the variability, some other factor is involved, as we know from the analytical results of Kraus and Goldsberry.

2. The isomorphism of bornite and chalcocite does not seem at all probable for there is not much similarity in chemical composition; moreover, one is isometric, the other orthorhombic.

3. The term morphotropy was introduced by Groth[5] to indicate the change in crystalline form brought about by substituting in a chemical compound an atom or group of atoms for a similar atom or group. The best examples of morphotropy are furnished by organic compounds. The only clear case of morphotropy (in its original restricted sense as distinct from isomorphism) among minerals is the humite group. Kraus and Goldsberry contend that there are various bornites each with a definite formula. As far as known bornite is isometric and, if so, it is difficult to conceive of any crystallographic change that can be brought about by the addition of the Cu_2S molecule. Complex formulæ like $Cu_{34}Fe_2S_{20}$, $Cu_{40}Fe_2S_{23}$ and $Cu_{76}Fe_2S_{41}$ are very improbable and it is probably a coincidence that the percentage compositions of the crystallized bornite from Bristol are so close to the theoretical values of $Cu_{10}Fe_2S_8$ and $Cu_{12}Fe_2S_9$.

4. In order to explain the variable chemical composition of bornite, the hypothesis of solid solution is advanced by the writer. As the lower limit of bornite seems to be Cu_3FeS_3, the variation in composition can be explained by assuming that *bornite is a solid solution of* Cu_2S *in* Cu_3FeS_3 which may be indicated thus: $Cu_3FeS_3(Cu_2S)_x$. This makes an indefinite upper limit for the copper content. There is proof that it is as high as $Cu_{12}Fe_2S_9$, and it probably goes still higher. The composition of bornites listed on page 547 of Kraus and Goldsberry's paper is as easily explained by the solid solution hypothesis as by any other and from a chemical standpoint it seems far more reasonable. The solid solution hypothesis also helps to explain the fact that chalcocite rarely occurs as an original hypogene[6] mineral for it seems probable that cuprous sulfoferrite (Cu_3FeS_3) can take up or dissolve appreciable amounts of cuprous sulfid, and hence chalcocite is not formed until a later stage when a change of conditions is brought about by decrease of temperature. It may also explain the readiness with which bornite alters to chalcocite. There is very little microscopic evidence to show that bornite and chalcocite are formed simultaneously except perhaps locally in the so-called intergrowths. The origin of these graphic intergrowths will be discussed by the writer in a forthcoming paper.

The long series of sulfo-salt minerals given by Kraus and Goldsberry fails to convince me of the general application of morphotropy in this group. Minerals represented by some of the formulæ in the series are doubtless examples of solid solutions, and some of these minerals are undoubtedly mechanical mixtures. Imagine what a bewildering lot of transparent minerals might have been recognized if mineralogists had had no microscopic check on their chemical work. A revision of the opaque sulfo-salt minerals seems necessary. In such work the metallographic microscope will be of great assistance.

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[5] *Pogg. Ann.*, 141, 31, 1870.

[6] This useful term is used by Ransome for minerals or ores formed by ascending solutions.

STUDIES IN THE MEASUREMENT OF THE ELECTRICAL CONDUCTIVITY OF SOLUTIONS[1]

It is a striking fact that very few investigators of conductivity have striven to make the measurements with an accuracy approach-

[1] This work was made possible by a grant from the Carnegie Institution of Washington to Professor S. F. Acree. New Orleans meeting of the American Chemical Society, April, 1915.

ing 0.01 per cent., and the writer has, with suggestions from Professor S. F. Acree, attempted to improve upon the technique employed by studying the conductivity cells, the baths, temperature regulation, the bridge and resistances, the methods of making and handling the solutions, and the so-called electrode and polarization phenomena. This improvement is now necessary in order to allow the writer to use the conductivity method for the determination of ionizations and reaction velocities in dilute solutions. He is indebted to Dr. Curtis[2] and Dr. Wenner, of the National Bureau of Standards, for much valuable advice, and the fine work of Washburn[3] and Bell shows what great improvements can be made in this line.

The writer has used the excellent equipment of the Bureau of Standards and some fine apparatus loaned us by Leeds and Northrup in making a fundamental study of a large number of factors, some of which have already been investigated in conductivity work by physical chemists. As a result of this work he has already greatly improved the methods and has studied: (1) the current from (*a*) induction coils, (*b*) a Holzer-Cabot wireless generator, (*c*) a General Electric Company large generator, (*d*) a Siemans-Halske generator for conductivity work, and (*e*) a Vreeland oscillator furnished by Leeds, Northrup & Co., which we have found to be the best source of current yet tried, as it gives a pure sine wave of uniform frequency which can be varied very widely; (2) the voltage, which when varied from 0.25 to 8 volts, has shown no influence on the resistance of the solutions measured so far *in very clean cells*, but is very important in cells not entirely clean; (3) the size and shape of the electrodes, which have a very large influence on the change of resistance and capacity of the cell with change in frequency; (4) the material used in making the electrodes (Pt, Au, Ag, Cu, Zn, etc.), which is very important; (5) the state of aggregation of the surface of the electrodes, as in plain, gray and platinized electrodes, which has a very great influence on the capacity of the cell and change of resistance with change in frequency; (6) the frequency of the alternating current, which when varied may change the resistance of some solutions in some cells as much as 3 per cent.; (7) the high capacity of the cell as a condenser, which is very important in decreasing the change of resistance with change in frequency and in obtaining a perfect minimum in the telephone; (8) the valence and velocities of the different ions; (9) the influence of the concentration and the character of the electrolyte and the solvent on the change of resistance with change in frequency; (10) the proper use of a condenser or inductance in balancing the capacity of the cell, and its influence on the resistance and minimum in the telephone; (11) the construction of the cell in such a way that no errors from evaporation and concentration can be produced; (12) the use of a tuned telephone attached to a stethoscope or of double wireless telephones; (13) the construction of a Wheatstone bridge with Curtis resistances free from inductance and capacity, kept automatically at constant temperature, and arranged so that every resistance can be checked against the others and against standard enclosed resistance; (14) the development of especially good constant temperature baths for such work; (15) the use of weight methods and special apparatus for making, keeping and transferring solutions; (16) a number of points connected with the proper use of all of the apparatus, especially the electrical equipment, to prevent errors arising from induction, capacity, skin effects, electrical leaks and other factors; (17) our criterion of excellent cells, namely that each one must be independent of the above sources of error and give readings constant to within 0.01 per cent., and especially that *whatever the solution used, the ratios of the resistances in any two such cells must be constant to within 0.01 per cent.* Only in this way can we be certain that the electrode effects have been practically eliminated and that we are measuring the true electrical resistance of the solution with great

[2] Curtis & Grover, Bureau of Standards Bulletin, Vol. 8, No. 3.

[3] *Jour. Am. Chem. Soc.*, 35, 177, 1913.

accuracy. By studying the electrode phenomena and other sources of error and correcting them we have now reached a precision of 0.001 per cent. and an accuracy of about 0.01 per cent. The details of all this work will appear shortly in another article.[4]

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PROCEEDINGS OF THE AMERICAN PHYSICAL SOCIETY

MINUTES OF THE SAN FRANCISCO MEETING

The seventy-eighth meeting of the American Physical Society was held at San Francisco, August 2 to 7, 1915. It was a joint meeting with Section B of the American Association for the Advancement of Science. The programs of the meeting on Tuesday, Wednesday and Thursday were in charge of the committee of the Pacific Coast division of the American Association for the Advancement of Science, of which Professor Fernando Sanford was chairman, and those of Friday were in charge of the Physical Society, President Merritt presiding. The meeting on Wednesday was held at Stanford University, Palo Alto. All other sessions for the reading of physics papers were held at the physical laboratory of the University of California, Berkeley. General sessions of the American Association for the Advancement of Science were held in San Francisco.

The following papers were presented:

Tuesday Afternoon—Spectroscopy

(1) "A Summary of the Leading Features of Electric Furnace Spectra"; (2) "The Spectrum of the 'Tube-arc' and a Comparison with Line Dissymmetries in Spark Spectra," by Arthur S. King.

"Review of Laboratory Studies of the Zeeman Effect, at Mount Wilson Solar Observatory," by Harold D. Babcock.

"Pole Effect in the Arc and Its Relation to Other Investigations," by Charles E. St. John and Harold D. Babcock.

"The Efficiency of Astronomical Spectrographs," by Joseph Moore.

Wednesday Afternoon (at Stanford University)

"Discussion and Demonstrations of High Potential Electric Currents," by Harris J. Ryan.

Thursday Forenoon and Afternoon—Physics of the Air

"The Thunderstorm," by W. J. Humphreys.

"New Concepts in Aërology," by A. G. McAdie.

"The Application of Physical Principles to Problems Suggested by Oceanic Circulation and Temperatures," by George F. McEwen.

"Radiation and the Atmosphere," by C. G. Abbot.

"Solar Radiation and Terrestrial Magnetism," by L. A. Bauer.

"On the Origin and Maintenance of the Earth's Negative Charge," by W. F. G. Swann.

"The Natural Charges of the Elements," by Fernando Sanford.

Friday Forenoon and Afternoon

"Thermo-electric Properties of Alloys of Bismuth and Tin," by A. E. Caswell.

"On the Free Vibrations of a Lecher System IV." (By title.) By F. C. Blake and Charles Sheard.

"Resistance of a Spark Gap," by W. P. Boynton.

"On the Resolving Power of Photographic Plates," by Orin Tugman.

"Sensitive Moving-coil Galvanometers," by Frank Wenner and Ernest Weibel.

"An Experimental Verification of the Law of Variation of Mass with Velocity for Cathode Rays," by Lloyd T. Jones.

"The Oxide Resistance Thermometer," by S. L. Brown.

"New Form of Radiation Pyrometer," by S. L. Brown.

"Electromotive Forces in Isothermal Metallic Circuits," by Gilbert N. Lewis.

"A New Method of Determining the Amplitude of Sound Vibrations in Air with Demonstration," by E. P. Lewis.

"An Application of the Koch Registering Microphotometer for Measuring the Sharpness of Photographic Images," by Orin Tugman.

"Photographic Study of the Tone of the Violin," by D. C. Miller.

"The Variation of the Photoelectric Current with the Angle of Emission," by Willard Gardner.

"A Quantitative Determination of the Earth's Penetrating Radiation," by C. H. Kunsman.

"Ultra-violet Absorption Spectra," by R. L. Sebastian.

"The Ultra-violet Spectra of Krypton and Xenon," by E. P. Lewis.

"The Law of Cohesion in Mercury," by P. A. Ross.

[4] See Taylor's address before the Physical Chemical Section of the American Chemical Society, New Orleans, April 1–3, 1915, and *Physical Review*, 6, 61 (1915).

"Note on the Theory of Ionization by Collision," by W. P. Roop.

"Heat Losses from Incandescent Filaments in Air," by L. W. Hartman.

"Magnetic Field Produced by Rotating Solid Conductors in a Magnetic Field." (Read by abstract.) By S. R. Williams.

Many physicists accepted the invitation to attend a joint meeting of Section A, the American Mathematical Society and the American Astronomical Society Tuesday forenoon to hear addresses on "The Human Significance of Mathematics," by C. J. Keyser, Columbia University, and "The Work of a Modern Observatory," by G. E. Hale, Mt. Wilson Observatory, Pasadena. Professor Hale's address was illustrated by interesting experiments on vortex motion.

Several instructive demonstrations were arranged by Professor E. P. Lewis, some of them at the request of Dr. Hale, where they could conveniently be examined between sessions. Among them were: Professor Stebbins's photoelectric cell for stellar photometry; the Zeeman effect with echelon grating, Fabry and Perot étalon and Lummer and Gehrecke plate; mercury fringes with Fabry and Perot interferometer; the amplitude of sound vibrations made visible by the forced vibrations of lycopodium particles.

Tuesday noon visiting physicists, astronomers and mathematicians and accompanying ladies were the guests of Professors E. P. Lewis, Haskell and Leuschner, at the luncheon at the Faculty Club, University of California.

Wednesday evening, immediately after the return from Stanford University, the physicists dined together at Jules Café, San Francisco. Attendance about thirty.

During the week many found opportunity to visit the exhibit of the National Bureau of Standards at the Panama-Pacific International Exposition, and some to make an excursion to the Lick Observatory at Mt. Hamilton, where the activities of the institution were explained by the astronomers in charge.

At the final session, a hearty vote of thanks was extended to the Pacific Coast Committee for the excellent arrangements made for the meetings, to the authorities of the University of California and of Stanford University for the accommodations provided and especially to the physics staff of the two institutions for the many courtesies extended by them.

A. D. COLE,
Secretary

ANNUAL MEETING OF THE AMERICAN GENETIC ASSOCIATION

THE American Genetic Association held its twelfth yearly meeting at Berkeley, Calif., August 2–6, in connection with the American Association for the Advancement of Science. More than three hundred persons attended the various conferences of the association.

The opening general meeting was held on Tuesday morning, August 3. President David Fairchild, of the U. S. Department of Agriculture, sent an opening address, in which he reminded the association that it had been organized to bring the message of genetics to the layman; to help the research worker to be more practical, and the practical breeder to be more scientific. He continued:

"The American Genetic Association is not primarily to promote research; it is to bring the biologist and the breeder together and help each to learn from the other. In my opinion, the greatest service we can do to genetics is to make its results available to the layman, and I hope to see the American Genetic Association more fully performing this service, year by year. I do not think we have fulfilled this obligation at all times as we should have done. It has been a constant temptation to coin new words, to invent methods of expressing our ideas in algebraical symbols, to present our researches in statistical form which made them a closed book to the practical breeder. All these methods are of use for the publication of original research, but in my opinion they must be supplemented by a simple account in plain English, for the benefit of those who are following our science, seeking its teaching for their own profit. They are calling on us to give them the light of science, and it is wicked to obscure this light by pedantry. I have no patience with those men of science who think their work loses dignity if it is put in simple English and made understandable to the layman. That was not the manner of Darwin, or of the other leaders of scientific thought in his generation; and if modern biology has less of a hold on the masses to-day than it had thirty years ago, if the teachings of biologists are less eagerly heard, I think we have ourselves largely to blame, and the custom which has insidiously grown on us, of describing our work in an esoteric terminology.

"I earnestly hope that the American Genetic Association can break away from this current, and stand forth as an exponent of real popularization of science. I believe the branch of science which

we represent is second to none in the importance which it has for society, and I therefore look on the growing tendency to lift this above the layman's comprehension as a calamity, in which I hope the *Journal of Heredity* will have no share."

Dr. Herbert J. Webber, of the University of California, who spoke on "Science in the Practise of Plant Breeding," remarked that the effect of the rediscovery of Mendel's Laws had often been over-emphasized. It had clarified our views, but as a fact, the segregation of characters in the second filial generation of a cross was well-known to breeders previous to 1900, and they used this knowledge constantly in their work. He emphasized the great opportunities offered to breeders by the immense number of possible combinations of unit characters, and declared that more geneticists should attack the great problem of the origin of variations—the fundamental problem of breeding, but one which most experimental breeders were neglecting. The pure line theory, he declared, offered a chance for reconciling the conflicting views of the selectionist and the hybridist. He urged that practical breeders should make themselves more familiar with morphology and cytology.

Rob R. Slocum, of the U. S. Department of Agriculture, presented a review of experimental work in poultry breeding, and declared that the results of this work did not materially modify the procedure which intelligent poultrymen had been accustomed to follow for many years. One of the greatest practical results of genetic research in poultry, he thought, was to encourage poultrymen to keep more accurate pedigrees of their fowls. His paper was illustrated by motion pictures.

E. D. Ball and Byron Alder, of the Utah Experiment Station, discussed the question "Is Egg-laying in the White Leghorn a Unit Character?" The results of their experiments at Logan, Utah, during seven years showed them that the first-year egg production of a hen is no reliable measure of what she will do in succeeding years, and that winter egg-production is not a proper measure of a hen's fecundity, being even more subject to environmental influences than yearly totals. They decided that no evidence hitherto presented by any one was adequate to answer the question whether egg-laying is a unit character.

Leon J. Cole and Frank J. Kelley, of the University of Wisconsin, described their experimental breeding work on dominant and recessive red in pigeons. The red color found in uniformly colored tumbler pigeons was found by Cole some years ago to be a simple Mendelian recessive to black. Another factor has been found, however, which has the capacity of altering the expression of black; so that birds carrying the factor for black, if they also carry this second factor, often have a distinct reddish appearance superficially resembling those individuals which are red because of the absence of black. This second factor is sex-linked.

W. S. Anderson, of the University of Kentucky, described his work in the investigation of horse breeding. Aside from the attack of such practical problems as sterility, he has investigated the lines of descent of the most famous American trotting stallions, and found that thousands "run out," to every one which shows on-breeding capacity. The importance of the dam was emphasized in this connection. Following Professor Anderson's paper, motion pictures of the horse breeding of the Bureau of Animal Husbandry, U. S. Department of Agriculture, were shown.

H. B. Frost, of the University of California, described mutation in *Mathiola annua*, a "Mendelizing" species, and reported on tests of pedigree-culture methods in Southern California.

B. O. Cowan, of Santa Monica, Calif., whose subject was "Inbreeding," concluded: "That inbreeding of live stock has brought very beneficial results can not be denied; that it is a source of danger is equally true; so if practised at all, it should be with the greatest discretion."

R. Ruggles Gates, of the University of London, spoke on "Successive Duplicate Mutations." Nilsson-Ehle first found duplicate and triplicate factors for red in wheat. Some races were found to have a single factor, giving only ratios 3 red: 1 white, others had two factors and hence gave also 15:1 ratios, while still others gave also 63:1 ratios and hence possessed three factors. It is suggested that this condition originated through mutation or chemical change having first taken place in one chromosome or pair of chromosomes. This gave the 3:1 condition. The duplicate condition arose from this afterwards, either through a similar change in another chromosome, or more probably by a mechanical re-mating of the chromosome pairs, thus giving 15:1 ratios. *Œnothera rubricalyx* similarly originated as a monohybrid through a chemical change in a chromosome, but some of the later generations have become dihybrid (giving 15:1 ratios) by a re-mating of the chromosomes. This rearrangement probably occurs at the time of fertilization rather than during meiosis.

In a second paper, Dr. Gates considered the modification of characters by crossing. Many writers hold that Mendelian characters always come out of a cross unmodified, although work by Davenport and by Castle and Phillips indicates that such is not always the case. A crucial instance of the modification of a character by crossing was furnished by various hybrids of *Œnothera rubricalyx* and *Œ. grandiflora.* In the F_2 of such crosses the red character *R* of the *rubricalyx* buds usually splits out sharply, but a few plants were intermediate in pigmentation, and in F_3 these bred true to the intermediate condition. Further, in (*rubricalyx* × *grandiflora*) × *grandiflora*, the depth of pigmentation of *R* plants is greatly diluted, though splitting takes place if the seed parent is heterozygous for *R.* The segregation is explained by the meiotic separation of the chromosome pairs. The dilution probably results from an inhibiting effect of the *grandiflora* chromosomes or perhaps from a modification of the *R* chromosome of *Œ. rubricalyx.*

A. D. Shamel, of the U. S. Department of Agriculture, spoke on the origin and development of the Washington navel orange, which he believes originated at Bahia, Brazil, nearly one hundred years ago, as a bud sport from a Portuguese variety. After a description of orange culture at Bahia and the introduction and dissemination of this variety in the United States, by the U. S. Department of Agriculture, Mr. Shamel described the origin of a number of distinct types in southern California, through bud mutation. It is believed that growers have tended to select the least productive, but most vigorous, of these sports for propagation, and the industry has therefore tended to deteriorate. Careful limb-selection of buds is now being practised, and the yield per acre is being much increased, while the character of the fruit is being improved, on the average.

E. J. Kraus, of Oregon Agricultural College, discussed self-sterility among orchard fruits. Careful observations have shown that poor production is often due to self-sterility, and that in general every variety must be tested, to find whether its own pollen is sufficient or whether it requires cross-pollination. If the latter proves to be the case, it must be tested with as many varieties as possible, to find under what conditions it succeeds best. The Oregon station has worked out detailed treatment for many of the leading varieties in its region.

In a second paper, Mr. Kraus took up the question of somatic segregation, as shown in certain varieties of pear.

The eugenics section met on the afternoon of August 3, in joint session with the American Social Hygiene Association and the Eugenics Research Association, David Starr Jordan presiding. The program, which was entirely furnished by the American Genetic Association, follows:

Irving Fisher, of Yale University, "Eugenics and Sociology." Professor Fisher discussed the mores, in their relation to eugenics, expressing a belief that the ideals of eugenics would come in some measure to be a substitute for the mores, as a criterion of morality and true value, when the race became more enlightened. At present, an action, or an institution or custom, is held to be desirable or undesirable, according as it does or does not agree with the folkways, the inherited, almost instinctive traditions of the race. In the future, people will rather ask, "Is its effect eugenic?"

Wilhelmine E. Key, of the State Training School, Polk, Pa., presented in abstract a paper on "Creating a Eugenic Conscience." She set forth conclusions based on three years of inquiry concerning eugenic ideals in all social grades. Study of extensive networks shows the operation of a fairly well-defined conscience to cut off degenerative lines and by the principle of segregation to enhance the efficiency of the better lines, in various directions. Even the embryo conscience, as illustrated by amusing instances, has worked toward this end. The rôle of eugenic laws should be to hasten the elimination of bad stock, rather than to interfere with the free choice of the average young man and woman. Too great stress on ideal fitness gives us the ingrowing eugenic conscience. It is here that feminism shows its baneful effect. Here, as elsewhere, the problem of right conduct is conditioned on right instincts. Just as current thought is becoming imbued with scientific conceptions, so we may expect that gradually the recognition of such principles as that of segregation in individual pedigrees, will lead to free conscious selection along a multitude of able lines. This will give predominance to the best-endowed strains, insure manifold variety and solve many of the problems of practical eugenics without the necessity of legal enactment, so far as society in general is concerned.

David Starr Jordan, of Stanford University, speaking on "The Long Cost of War," emphasized the reversal of natural selection which takes place

in warfare under modern conditions, and described the effect of this dysgenic factor on modern history.

Samuel C. Kohs, of the House of Correction, Chicago, taking as his subject "Eugenics and the Unconscious," warned those doing research in the heredity of human psychical traits that they were in many cases wholly superficial, and that definition of the traits which they discussed was a prerequisite of intelligent treatment. He then described some of the recent studies of the unconscious mind, which indicate that many traits in children, which are commonly believed to have been inherited from parents, might in reality be due rather to impress on the unconscious mind, during the early years of childhood. Most of the work on the inheritance of mental characters in man is of doubtful value, he declared, because of any one or more of the following reasons:

1. Inaccurate tools with which to measure the ability or capacity.

2. Amateur field workers.

3. The use of the questionnaire method.

4. Where more than one field worker was necessary for obtaining the data, the differences in the individual standards of the field workers vitiated the results.

5. Being told for what to look, and possessing the popular conceptions regarding the inheritability of all sorts of traits, it is only just to assume that many of the assistants very easily found what was not there.

6. The study of character and personality is still in its infancy. To assume that certain peculiarities are due to the presence or absence of specific determiners can, in our present state of knowledge, hardly be substantiated by actual facts.

7. Some students approach the inheritance of *mental* traits too much from a biological point of view, and therefore go astray.

A. J. Rosanoff and Helen E. Martin, of the Kings Park State Hospital, Long Island, N. Y., submitted a preliminary report of a study on the offspring of the insane, which indicates that the forms of insanity considered behave as Mendelian recessives.

Ethel H. Thayer, of the Mendocino State Hospital, Talmage, Calif., described some of the cacogenic problems of California. She mentioned that the state sterilization law is now almost inoperative, because defectively drawn so that it can not be applied to the feeble-minded, the most important of the cacogenic classes which come under its scope.

Surgeon W. C. Billings, of the U. S. Public Health Service, Angel Island (San Francisco), Calif., described in some detail the administration of the immigration laws at California ports. Nearly all the immigrants are Asiatics, and therefore offer little of importance to eugenics, because marriages between them and the white population of the United States are extremely rare.

Walter B. Swift, in charge of the Voice Clinic of the Boston State Hospital, spoke on the possibility of voice inheritance. He discussed the inheritance of bone forms and body shapes as a basis upon which to build. The transmission of bone cavities as a further foundation. Consideration of the Indians' "high cheek bones" and the straight front nose of the Greek. The equine nose as found in the Jew. Such transmissions of bony exteriors—as an indication that cavities they contain are also inherited at least in some measure. If cavities are inherited then vocal elements based upon cavity formation for their fundamental qualities and overtones may also possibly be transmitted. Evidence from other sources. Illustrative cases.

The plant breeding section met all day on August 5, hearing the following papers:

Ernest B. Babcock, University of California, described walnut mutant investigations. "In 1912 I discovered an apparently normal tree of the California black walnut which annually bears a good crop of nuts, most of which when planted produce typical black walnuts, but a few of which produce a new type of walnut which I have named *quercina* because of its general resemblance to a small-leaved oak. This tree is probably the only perennial mutating individual accessible for experimentation. By using root-tips from these two types of seedlings we have ascertained the number of chromosomes characteristic of each and that the number is the same for both, thirty-four. Hence the mutation must be due to some other cause than a change in chromosome number. Breeding experiments are under way which may throw light on the nature of this mutation."

L. D. Batchelor, University of California, explaining problems in walnut breeding, pointed out that most of the Persian (English) walnut groves of California are composed of seedlings, and that these must be worked over to the best strains, if the industry is to have its maximum efficiency.

Howard Gilkey, of the University of California, emphasized the need for breeding ornamentals that would meet the landscape gardener's requirement of definite types of form.

Francis E. Lloyd, of McGill University, described his study of the Japanese persimmon, in which he found the presence of an emulsion colloid, which when it coagulated not only fixed the tannin, thus making the fruit palatable, but also caused a change in color of the flesh. In a second paper on "Intra-ovarial Treatments: Methods and Results," he said: "In pursuance of earlier investigations, and in the hope of reaching the egg-cell directly by means of reagents which might possibly permanently disturb the germ-plasm, *Torenia Fourneri* has been studied. In spite of the protuberant embryo-sac, bringing the egg-apparatus into a position of apparently maximum exposure, its position in contact with the placenta, together with other mechanical relations, precludes the object sought. The course of reagents (using methylene blue as a criterion) injected into the placenta is essentially identical with that earlier described for *Scrophularia* (Carn. Inst. Wash., Ann. Rep. 1914, p. 77). It has further been discovered that the mutual adjustments of the elements of the egg-apparatus and embryo-sac, which show a diffusion pressure equivalent to that of a 0.1*N* KNO3 solution, are so delicate that access to tap-water is followed by bursting of the synergidæ and partial extrusion of the hydrocellulosic beak material, thus precluding the use of dilute watery solutions in immediate contact with the embryo-sac."

Sarkis Boshnakian, of Cornell University, described a new checkerboard method of representing Mendelian segregation, and gave a coefficient of squarehead form necessary for the statistical study of density in wheat.

W. B. McCallum, of San Diego, Calif., described the cultivation of several million plants of guayule (*Parthenum argentatum*), a Mexican plant which produces rubber. Although regarded as a single species, the plant has been found to have at least 125 forms, varying widely and all breeding true.

E. F. Gaines, Washington State Experiment Station, gave a brief account of results obtained by crossing wheats and barleys differing in two or more unit factors. Both dominance and lack of dominance have been secured in different cases, and one case of triplicate identical factors, cumulative in effect, was reported.

C. C. Vincent, Idaho Experiment Station, made a preliminary report on apple-breeding projects at that station. He told of the need of new varieties and described some thousands of crosses that have been made.

H. E. Knowlton, of Cornell University, describing studies in pollen germination with special reference to longevity, reported that pollen of the snapdragon (*Antirrhinum majus*) remained viable longest when kept at low temperature. Pollen stored at —17° to —23° C. for six weeks gave a fair percentage of germination in sugar solution, and moreover some of the flowers pollinated with it produced seeds.

Arthur W. Gilbert, Cornell University, "Color Inheritance in *Phlox drummondi.*" "The following unit characters were found in the four varieties of *Phlox drummondi* that were used in these experiments: (1) A dark eye factor producing a dense coloration at the center of the flower. This was dominant over its absence, the white eye, which was exhibited in more or less of a definite pattern. (2) A blue factor. (3) A red factor. (4) An intensifying factor which determines the degree of pigmentation of the reds. (5) A yellow factor which acts only in the presence of the eye factor. The reds and blues are cell-sap colors, and the yellow is due to the presence of yellow chromoplasts."

Alfred C. Hottes, of Cornell University, discussed the practical hybridization of the gladiolus. It is a genus of about one hundred and thirty species, mostly natives of South Africa, a few from Europe. Approximately fifteen species have been cultivated or used in hybridization, so that this flower offers an excellent example of a flower improved by the incorporation of a number of species. The work has been carried on chiefly without reference to laws of inheritance; each species has transmitted to some hybrid a desirable feature which has been selected and impressed upon other hybrids.

George F. Freeman, of the University of Arizona, "Inheritance of Quality in Wheat." A number of recent investigators have declared that wheat quality is dependent on environment, and that the breeder could not control it. Careful review of all the work done shows this conclusion to be erroneous. Qualitative factors in wheat are to a large degree dependable and controllable.

John W. Gilmore, of the University of California, illustrated the wide variability of rye grasses and told of the possibilities for the practical breeder.

George L. Zundel, Cornell University, spoke on disease resistance in celery. Preliminary experiments were carried on at Cornell University by the writer to test the relative susceptibility of varieties of celery to the fungus *Septoria petroselini* Desm. var. *apii* Br. et Cav. No variety was

found to be resistant to the fungus, but individual plants were found that were nearly immune. Since the celery flower is self-pollinated a method for future work is at once suggested, *e. g.*, the selection of the immune plants as parents upon which to build immune strains of celery.

In a second paper, Mr. Zundel discussed the evolution of celery. Celery has been known to mankind for centuries. The Greeks and Romans used it mostly as a medical plant. They attributed to it great curative powers. Its native habitat is given as ranging from Sweden to the Mediterranean and into British India. It is known botanically as *Apium graveolens* L. The early English name for celery was smallage and later it was known as salary. The Greeks called it *Elioselinon* or marsh parsley. Abercombie, in 1778, gives the first list of named varieties of celery. He gives four varieties, all of which originated on the large estates of titled gentlemen. Celery was at this time regarded as a luxury for the tables of titled gentlemen. The introduction, about 1883, of the Golden Self Blanching and White Plume celery revolutionized the celery industry of America.

G. P. Rixford, of the U. S. Department of Agriculture, told of the pistachio nut, which he believes will become a crop of some importance in parts of the south and west.

C. O. Smith, of the University of California, gave particulars of a method of inoculating plants to determine their comparative resistance to disease, in breeding work.

Frank S. Harris and J. C. Hogenson, Utah Agricultural College, discussed some correlations in sugar beets. It was found that the larger beets had the smaller sugar content; a number of other correlations were cited, which facilitate beet breeding.

G. N. Collins and J. H. Kempton, of the U. S. Department of Agriculture, described a bigeneric grass hybrid (*Tripsacum dactyloides* × *Euchlæne mexicana*) which shows no trace of the influence of the seed parent. Some reasons were given for thinking that it is not parthenogenetic.

W. A. Setchell and T. H. Goodspeed, of the University of California, conducted the meeting on a tour of their tobacco-breeding experiments.

At the closing general session of the association, Friday afternoon, August 6, Mrs. Myrtle Shepherd Francis, of Ventura, Calif., related her experience in breeding double seeding Petunias, and exhibited specimens.

H. Hayward, Delaware Experiment Station, spoke on inbreeding. Pearl's method for measuring accurately the degree to which an animal is inbred has caused a revision of many ideas on the subject. It has been found, for instance, that some of the famous sires of Bates, Booth and the Collings, were not nearly as much inbred as is popularly supposed. Breeding experiments with pigs, conducted at the Delaware station under careful control, have satisfied the speaker that when inbreeding is carried beyond a certain point, deterioration is inevitable. It is difficult to fix any arbitrary point, however, as the limit of safety.

Isabel McCracken, of Stanford University, described Mendelian breeding experiments with silkworms.

J. H. Kempton, of the U. S. Department of Agriculture, described the result of a long series of breeding experiments with maize.

Albert F. Etter, of Briceland, Calif., told of his work in strawberry breeding, in which he has crossed commercial varieties with the beach strawberry of the Pacific coast, with the alpine species, and others. Plants much more resistant and productive than any present commercial variety have been obtained, and the berries offer a wide range of desirable commercial characters.

C. L. Redfield, of Chicago, defended his theory of dynamic evolution, maintaining that such functional qualities as speed in race horses, or milk-production in cows, are developed by work and that the results of this development are then transmitted to the offspring. He declared that no single instance has ever been cited where this rule was violated.

C. L. Lewis, of the Oregon Agricultural College, speaking on plant-breeding problems of the Pacific Coast, declared it was a mistake to think there were plenty of good varieties of fruit already in existence; that in nearly every field the genetist was needed. He cited many cases to prove his point.

It was decided to continue holding the meetings of the American Genetic Association in connection with those of the American Association for the Advancement of Science. A committee headed by Herbert J. Webber and comprising R. Ruggles Gates, George H. Shull, W. E. Castle, Raymond Pearl, H. S. Jennings and Paul Popenoe, was appointed on nomenclature, with the particular request that it consider suitable definitions of inbreeding and linebreeding, which could be agreed on by genetists and practical breeders, and relieve the confusion which now attends the use of these two words.

Paul Popenoe,
Secretary pro tem.